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George B. Kistiakowsky

The four anniversaries

The *Bulletin* dedicates this issue to three major events in the exploration of the atomic nucleus and their consequences to all us of whose fiftieth, fortieth and thirtieth anniversaries fall due this year. I will add the fourth, cheating on the timing a bit because to me it did happen 60 years ago.

As a woefully unprepared student I was admitted in 1921 to the University of Berlin after years of soldiering in the Ukraine and then living by manual labor in the Balkans. Somewhat later I started attending the University physics colloquium, the like of which probably will never happen again.

Led by Einstein and Planck, the whole contingent of senior physicists (and near-physicists like Franz Simon, later of Oxford) assembled there. They were joined by colleagues from the State Technology Institute and the top staff of the German Bureau of Standards, led by Walter Nernst, its director and to all but his close friends, *Herr Wirklicher Geheimrat*, as if the Kaiser was still sitting on the throne. Leaders of one of the local Kaiser Wilhelm (much later the Max Planck) Institutes—F. Haber and M. Polanyi, among others—filled the remainder of the front rows of the auditorium. We, the beginning students, sat way back.

My first recollection of—and my first encounter with—the atomic nucleus was a report about the latest published findings of Rutherford on collisions of fast alpha particles from naturally radioactive elements with light atoms. Nernst, never shy, immediately expressed his emphatic distrust of Rutherford's claims to have achieved artificial disintegration, because beta particles, though much faster than alphas, were thought to be incapable of producing such effects. A most naive remark, noted Einstein, since velocity is secondary and the alpha particles have far greater kinetic energy. In that case, said Nernst triumphantly, why does not Rutherford do it with rifle bullets? Einstein ended the dialogue with a loud remark about regrettably common illiteracy in physics, which included me, since only later was I was able to determine who had had the last word. It was not the only such exchange between these two, who so thoroughly detested each other.

First Congress of International Physicians for the Prevention of Nuclear War. (From left) George Kistiakowsky; Georgi Arbatov, USSR; Evgenyi Chazov, M.D., USSR; Bernard Lown, M.D., President of International Physicians.

Some ten years later and 50 years ago, Chadwick and Feather, working in the Cavendish Laboratory, established that free neutrons were produced in the collisions of alpha particles with nitrogen. This was a finding that had eluded the Joliot-Curies, who were also working on particle production in alpha particle collisions. It was a major event in the midst of a great flowering of nuclear physics, a pure science that in the words of Rutherford would never find major practical applications.

But only a few years later, Hahn and Strassman repeated and extended Enrico Fermi's work on collisions of neutrons with uranium, finding barium among the products. Frisch and Meitner concluded that a hitherto undiscovered phenomenon, the fission of the uranium nucleus into lighter elements and free neutrons had taken place. At about that time Leo Szilard pointed out that if more than one neutron resulted from each uranium fission, a self-sustaining and even a self-multiplying reaction was in

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principle possible. There was of course a close analogy here to the branching chain reactions of chemical kinetics known since the mid-1920s from the work of N.N. Semenoff and his collaborators.

These events signaled the end of nuclear physics as a pure and abstract science. Under the spur of World War II, Adolf Hitler's own bloody chain reaction, the first self-sustained nuclear chain reaction took place just 40 years ago at the University of Chicago—the work of Fermi and a talented team. Less successful work toward the same objective was underway in Germany and the Soviet Union. To the men in power in Washington who provided the money and wartime priorities, the result was not a triumph of science and engineering but a way to manufacture the plutonium deemed necessary to make atomic weapons, and so the first atom bombs were American. Thus began the alliance of nuclear physics and what later became known as the military-industrial complex.

The tool of the alliance, the Atomic Energy Commission with its ever growing bureaucracy, was shielded from criticism and even inquiries by the impenetrable wall of special "restricted" security laws. It soon became one of the most singleminded and arrogant agencies in Washington. While ostensibly civilian-controlled and dedicated not only to weapons but to nuclear electric power as well, the latter became the stepchild while the Commission was allied with and infused by the Pentagon.

Nowhere else did the prostitution of scientific integrity match that of the Atomic Energy Commission's technical staff, including the weapons laboratories and extending to its present-day successors. And all for the sake of building more numerous and more lethal nuclear warheads. Remember the rigging of the Oppenheimer "trial"; the perjured testimony about the harmlessness of the fallout from Nevada surface test explosions, with thousands of dead sheep only a short distance downwind; the highly dubious means that were used to block President Eisenhower's efforts to reach a comprehensive test ban treaty; and later, the repeated diversion of funds specifically voted by Congress to advance the safety of nuclear power reactors. The list could go on and on.

"I am sure that at the end of the world—in the last millisecond of the Earth's existence—the last human will see what we saw."

George B. Kistiakowsky
Trinity Test, July 16, 1945

The outcome is that the American nuclear power industry is in shambles. Its influence on world development of nuclear power is lost and recovery is far in the future. But the weaponeers are rampant, thanks largely to the event that followed the anniversary being commemorated by the *Bulletin*. Thirty years ago the concept of two-stage fission-fusion weapons was developed by Teller and Ulam, and the Los Alamos Laboratory made it a reality. The initial enthusiasm of the weapons developers and their allies in the military related to readily transportable multi-megaton monsters (remember Khrushchev's 60-megaton pride and joy). But time showed that the really important factor was the flexibility which allowed the weaponeers to cut and vary the weight-to-explosive yield ratio, the physical shape, the ratio of radiation to blast, and so on.

Unlimited opportunities arose for fitting nuclear warheads to ever more sophisticated means of delivery against the potential enemy. And the enemy, by a carefully nurtured tradition, reinforced from time to time by misinformation fed to the public and to Congress, was identified as the Soviet Union. The Soviets, of course, kept up with us in most respects. And so here we are, possessors of some 50,000 nuclear warheads: more than enough to produce a holocaust that will not only destroy industrial civilization but is likely to spread over the Earth environmental effects from which recovery is by no means certain.

The political leaders of powerful nations continue uttering pious words about their love of peace, but the arsenals keep growing, the stability of nuclear peace is being undermined, and the proposals for arms controls negotiations on both sides are so unbalanced as to be obviously non-negotiable.

As one who has tried to change these trends, working both through official channels and, for the last dozen years, from outside, I tell you as my parting words: Forget the channels. There simply is not enough time left before the world explodes. Concentrate instead on organizing, with so many others who are of like mind, a mass movement for peace such as there has not been before. But the threat of annihilation is also unprecedented. Then lead the movement so that, instead of the few now in Washington, many will be elected to Congress who have a true and unbreakable commitment to search for peace. □

DECLARATION ON PREVENTION OF NUCLEAR WAR

I. PREAMBLE Throughout its history, humankind has been confronted with war, but since 1945 the nature of warfare has changed so profoundly that the future of the human race, of generations yet unborn, is imperiled. At the same time, mutual contacts and means of understanding between peoples of the world have been increasing. This is why the yearning for peace is now stronger than ever. Mankind is confronted today with a threat unprecedented in history, arising from the massive and competitive accumulation of nuclear weapons. The existing arsenals, if employed in a major war, could result in the immediate deaths of many hundreds of millions of people, and of untold millions more later through a variety of after-effects. For the first time, it is possible to cause damage on such a catastrophic scale as to wipe out a large part of civilization and to endanger its very survival. The large-scale use of such weapons could trigger major and irreversible ecological and genetic changes, whose limits cannot be predicted.

Science can offer the world no real defense against the consequences of nuclear war. There is no prospect of making defenses sufficiently effective to protect cities since even a single penetrating nuclear weapon can cause massive destruction. There is no prospect that the mass of the population could be protected against a major nuclear attack or that devastation of the cultural, economic and industrial base of society could be prevented. The breakdown of social organization, and the magnitude of casualties, will be so large that no medical system can be expected to cope with more than a minute fraction of the victims.

There are now some 50,000 nuclear weapons, some of which have yields a thousand times greater than the bomb that destroyed Hiroshima. The total explosive content of these weapons is equivalent to a million Hiroshima bombs, which corresponds to a yield of some three tons of TNT for every person on Earth. Yet these stockpiles continue to grow. Moreover, we face the increasing danger that many additional countries will acquire nuclear weapons or develop the capability of producing them.

There is today an almost continuous range of explosive power from the smallest battlefield nuclear weapons to the most destructive megaton warhead. Nuclear weapons are regarded not only as a deterrent, but there are plans for their tactical use and use in a general war under so-called controlled conditions. The immense and increasing stockpiles of nuclear weapons, and their broad dispersal in the armed forces, increase the probability of their being used through accident or miscalculation in times of heightened political or military tension. The risk is very great that any utilization of nuclear weapons, however limited, would escalate to general nuclear war.

The world situation has deteriorated. Mistrust and suspicion between nations have grown. There is a breakdown of serious dialogue between the East and West and between North and South. Serious inequities among nations and within nations, shortsighted national or partisan ambitions, and lust for power are the seeds of conflict which may lead to general and nuclear warfare. The scandal of poverty, hunger, and degradation is in itself becoming an increasing threat to peace. There appears to be a growing fatalistic acceptance that war is inevitable and that wars will be fought with nuclear weapons. In any such war there will be no winners.

Not only the potentialities of nuclear weapons, but also those of chemical, biological and even conventional weapons are increasing by the steady accumulation of new knowledge. It is therefore to be expected that also the means of non-nuclear war, as horrible as they already are, will become more destructive if nothing is done to prevent it. Human wisdom, however, remains comparatively limited, in dramatic contrast with the apparently inexorable growth of the power of destruction. It is the duty of scientists to help prevent the perversion of their achievements and to stress that the future of humankind depends upon the acceptance by all nations of moral principles transcending all other considerations. Recognizing the natural rights of the human race to survive and to live in dignity, science must be used to assist humankind toward a life of fulfillment and peace.

Considering these overwhelming dangers that confront all of us, it is the duty of every person of good will to face this threat. The disputes that we are concerned with today, including political, economic, ideological and religious ones, are not to be undervalued but lose their urgency when compared to the hazards of nuclear war. It is imperative to reduce distrust and to increase hope and confidence through a succession of steps to curb the development, production, testing and deployment of nuclear weapons systems, and to reduce them to substantially lower levels with the ultimate hope of their complete elimination.

To avoid wars and achieve a meaningful peace, not only the powers of intelligence are needed, but also the powers of ethics, morality and conviction.

The catastrophe of nuclear war can and must be prevented. Leaders and governments have a great responsibility to fulfill in this regard. But it is humankind as a whole which must act for its survival. This is the greatest moral issue that humanity has ever faced, and there is no time to be lost.

II. In view of these threats of global nuclear catastrophe, we declare:

• *Nuclear weapons are fundamentally different from conventional weapons. They must not be regarded as acceptable instruments of warfare. Nuclear warfare would be a crime against humanity.*

• *It is of utmost importance that there be no armed conflict between nuclear powers because of the danger that nuclear weapons would be used.*

• *The use of force anywhere as a method of settling international conflicts entails the risk of military confrontation of nuclear powers.*

• *The proliferation of nuclear weapons to additional countries seriously increases the risk of nuclear war and could lead to nuclear terrorism.*

• *The current arms race increases the risk of nuclear war. The race must be stopped, the development of new more destructive weapons must be curbed, and nuclear forces must be reduced, with the ultimate goal of complete nuclear disarmament. The sole purpose of nuclear weapons, as long as they exist, must be to deter nuclear war.*

III. Recognizing that excessive conventional forces increase mistrust and could lead to confrontation with the risk of nuclear war, and that all differences and territorial disputes should be resolved by negotiation, arbitration or other peaceful means, we call upon all nations:

• *Never to be the first to use nuclear weapons;*

• *To seek termination of hostilities immediately in the appalling event that nuclear weapons are ever used;*

• *To abide by the principle that force or the threat of force will not be used against the territorial integrity or political independence of another state;*

• *To renew and increase efforts to reach verifiable agreements curbing the arms race and reducing the numbers of nuclear weapons and delivery systems. These agreements should be monitored by the most effective technical means. Political differences or territorial disputes must not be allowed to interfere with this objective;*

• *To find more effective ways and means to prevent the further proliferation of nuclear weapons. The nuclear powers, and in particular the superpowers, have a special obligation to set an example in reducing armaments and to create a climate conducive to non-proliferation. Moreover, all nations have the duty to prevent the diversion of peaceful uses of nuclear energy to the proliferation of nuclear weapons;*

• *To take all practical measures that reduce the possibility of nuclear war by accident, miscalculation or irrational action;*

• *To continue to observe existing arms limitation agreements while seeking to negotiate broader and more effective agreements.*

IV. Finally, we appeal:

• *To national leaders, to take the initative in seeking steps to reduce the risk of nuclear war, looking beyond narrow concerns for national advantage; and to eschew military conflict as a means of resolving disputes.*

• *To scientists, to use their creativity for the betterment of human life and to apply their ingenuity in exploring means of avoiding nuclear war and developing practical methods of arms control.*

• *To religious leaders and other custodians of moral principles, to proclaim forcefully and persistently the grave human issues at stake so that these are fully understood and appreciated by society.*

• *To people everywhere, to reaffirm their faith in the destiny of humankind, to insist that the avoidance of war is a common responsibility, to combat the belief that nuclear conflict is unavoidable, and to labor unceasingly towards insuring the future of generations to come.* ☐

September 24, 1982

Presented to His Holiness the Pope by an assembly of scientists convened by the Pontifical Academy of Sciences

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BERNARD T. FELD

The year of appeals

By coincidence, this year provides the occasion for a multiple celebration of some of the most significant events of this nuclear era. That these landmarks are not being visibly celebrated speaks loudly for the ambivalence of our contemporary scientific community regarding its antecedents.

Fifty years ago, in 1932, James Chadwick discovered the neutron. With this discovery, the way was opened for a possible release of the energy locked up in the atomic nucleus, whose formidable charge renders it effectively impermeable to penetration by charged bullets, until then the only means available for nuclear reaction studies.

The breakthrough occurred at the end of 1938, in Germany, just as the world was descending into the abyss of World War II. Otto Hahn and Fritz Strassman, pursuing the leads provided by Fermi and others as to the puzzling consequences of neutron absorption in uranium, discovered the fission process.

Thus the stage was set for the next great step—the second landmark, in 1942, in Chicago, where Fermi, Szilard and their co-workers achieved the first man-made nuclear chain reaction. December 2, 1982 is the fortieth anniversary of this event.

In 1957 (25 years ago) the Russell-Einstein Manifesto appeared. It was, in effect, a proclamation by the world scientific community of its commitment to end the East-West confrontation and to begin serious and persistent discussions among scientists from both sides on means of eliminating the threat of nuclear disaster.

The production and deployment of nuclear weapons, if at all justifiable, can have only one possibly valid purpose—the prevention of their use under all circumstances. Stating it in the language of deterrence highlights the inherent contradiction of the doctrine of mutual assured destruction (MAD), not to speak of the current doctrines of limited or winnable nuclear war, as well as the fundamental moral dilemma that it presents. The worldwide spread of the no-first-use movement is in response to this dilemma—an attempt to broaden and deepen the firebreaks inhibiting the outbreak of a nuclear war.

The most recent manifestation of this concern was the meeting of the Pontifical Academy of Scientists at the Vatican on September 24 of this year. Attended by representatives of the world's major academies and societies of science, the meeting issued a declaration on the dangers of nuclear war and measures for preventing it. We must pause to remember the words of the Russell-Einstein declaration of 1957 and read carefully the declaration of 1982. We must hope that political leaders of the nuclear weapons nations will respond through serious and positive political action.

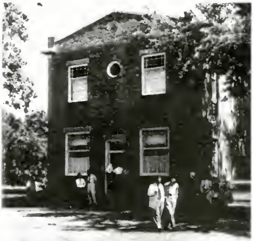
Thus far, the movement is mostly "elitist"—originating primarily with groups of professionals—although there is evidence of a large and growing support from the "grass roots" as well.

Some of the most effective and widely supported of such appeals originate in Japan, the first and only victim of nuclear attack. For example, shortly before his death last year, Hideki Yukawa, Nobel Laureate in Physics and one of the original signers of the Russell-Einstein Manifesto, and his wife Sumi initiated an appeal that by early this year had already attracted over 300,000 signatures. This is both an appeal to the outside world "to abolish all nuclear weapons completely from the earth" and an admonition to their countrypeople that "we Japanese have to make up our mind not to have nuclear weapons ."

Such an appeal has been reiterated by a large part of the Japanese scientific community, as, for example, in "We Must Seek Peace—Now," a statement by over 4,000 Japanese scientists, calling for "the abolition of nuclear weapons and the prevention of wars," issued by the Fourth Kyoto Conference of Scientists, in June 1981. Another statement on "the peril of nuclear war and [for] nuclear disarmament" was adopted at the 85th General Meeting of the Science Council of Japan on May 21, 1982.

In the forefront of the scientists' peace movement have been the Pugwash Conferences on Science and World Affairs, whose twenty-fifth anniversary meeting was held in Warsaw at the end of August.

The statement adopted at this Conference—and endorsed by an overwhelming majority of living Nobel laureates in the natural sciences—typifies the appeals that have been issued by various professional groups, most of which, until recently, have been reluctant to involve themselves directly in the political process. Thus, at a meeting in Geneva on June 18, 1982, the executive committee of the European Physical Society called on "those statesmen engaged in the Geneva talks to bring an end to the



The schoolhouse in Pugwash, Nova Scotia, where the first Pugwash Conference on Science and World Affairs was convened in 1957.

arms race and to bring about a reduction in the level of nuclear armaments as a first step for creating that atmosphere of political trust in which all means of nuclear warfare can be abolished. . . ."

A similar appeal, emanating from over 200 members of the U.S. National Academy of Sciences, was reported in the June issue of *Physics Today*. And, signed by 350 staff members of the European Community Joint Research Center at Ispra, Italy, a statement entitled "We denounce the nuclear arms race," called on "the European Parliament and . . . all the governments of member states to promote every action possible to set discussions in motion and to halt the arms race."

Other recent developments are the statements issuing from special professional schools and institutes. For example, more than 65 physicists—about 80 percent of those attending the Cargèse (Corsica) NATO Advanced Study Institute on "Fundamental Interactions" last summer—appealed to NATO

"to implement . . . the agreement on negotiations for a mutual reduction of nuclear weapons and to commence these negotiations immediately."

The same appeal was endorsed by participants at summer schools in Les Houches, France, and Edinburgh, Scotland. And a similar one was adopted at the International School of Plasma Physics in Varenna, Italy.

An interesting variant was an appeal made jointly by a group of farmers and some 50 scientists from international institutions in the region of the Larzac Plateau, that part of France bordering on Switzerland and near Geneva. Entitled "Science, Like the Larzac Plateau, Should be Demilitarized," it warned scientists not "to be accomplices of a society that crushes men or that may very well destroy them," and appealed to the public to

"find a way of making scientists work only towards the production of goods that are socially useful; ask the present government to become fully involved in disarmament and to have research done on defense means other than arms; and clearly manifest its solidarity with those scientists who are ready to take the risk of jeopardizing their career by refusing to participate in the preparation of a third world war."

Another "Scientists Appeal Against the Nuclear Arms Race," originating in Paris, expresses the wish "to really contribute towards the end of the arms race whose main proponents are the U.S. and the U.S.S.R." but it also notes that "first of all, in France we will oppose all new arms programs (multiple warhead missiles, new generations of submarines, the neutron bomb), as well as the pursuit of an exportation policy that favors proliferation."

With respect to nuclear arms, of course, France—with her independent *force de frappe*—is in a very different situation from the rest of Europe. Thus, the growing European Nuclear Disarmament (END) Campaigns, while primarily focusing political pressures on the West European governments, are essentially aimed at NATO nuclear policy and at its main driving force, the United States. In particular, the recent decision to deploy a new generation of Pershing II and cruise missiles in Europe—in response to Soviet deployment of the rather sophisticated and mobile SS-20—is believed by most West European peace activists to signal an unnecessary escalation of a competition already characterized by massive overkill. Thus, an "Appeal from Berlin to the People of the U.S.A." (New York Times, April 10, 1981), asks: "Who can seriously believe the deployment of Pershing II and cruise missiles in Europe will strengthen the prospects of peace?" It goes on to state: "We, along with millions of other European citizens, will never accept additional nuclear weapons in Europe."

Nor have East European scientists been immune to these concerns. Though it is generally accepted in the West that statements from the East have a somewhat more official character, it is difficult to avoid the conclusion that the East European scientific community shares the concerns of its Western colleagues. Thus, an appeal from the Academies of Sciences of the Socialist Countries, meeting in Sofia, Bulgaria, in December 1981, calls on the scientists

"to come forward in defense of universal human values, peace for all and social progress; against the arms race; for holding a constructive dialogue between states to settle all international issues by negotiations only; and for strengthening and continuing the policy of detente and peace."

Bernard T. Feld, professor of physics at MIT, is editor-in-chief of the *Bulletin*.

The East European academies recognize that safety in the nuclear age.

"cannot be achieved by counting on military superiority or on victory in a nuclear war. Real safety is the safety of the entire world community . . . without an equitable dialogue and business-like negotiations it is impossible to solve the international problems of the contemporary world, however acute and complex they may be."

At the forefront of this international movement have been the physicians, through the International Physicians for the Prevention of Nuclear War. Its Second Congress, held in Cambridge, England, early in April 1982, issued an appeal to the physicians of Europe to join them in their efforts "to avert the greatest danger which the world has ever faced." Particularly, the European physicians are asked to:

"1. Join the medical organization against nuclear war in your own country. . . .

2. Inform yourself about the nuclear arms race. . . .

3. Institute courses on nuclear war in your medical schools.

4. Present the facts . . . to governmental leaders and to the public. . . .

5. Prepare articles and make the problem of prevention of nuclear war a subject of special discussion in medical societies, at symposia and at conferences.

6. Preserve the tradition of open and unfettered exchange between physicians and scientists of many countries . . . which may facilitate cooperative international efforts toward nuclear disarmament."

Furthermore, the Congress issued "an appeal to the President of the United States of America, Ronald Reagan, and to the Chairman of the Presidium of the USSR Supreme Soviet, Leonid Brezhnev." It stressed that, in a nuclear war:

"1. There is no possibility of an effective medical response to the ensuing chaos. . . .

2. Those who might survive the initial effects . . . would face the prospect of prolonged agony and slow death . . . and the social, cultural, environmental and medical damage . . . would persist for generations. . . .

3. Nuclear war . . . would destroy . . . the achievements of thousands of years of human effort. . . .

4. Since physicians would have no remedy for the foreseeable medical consequences of a nuclear war, the only effective action is prevention. . . . The imperative of our time is to rule out the very idea of the use of nuclear weapons, in any form or on any scale."

Penetration of the physicians' movement into East Europe was assured by the involvement in the International Physicians organization of Dr. Evgenyi Chazov, Leonid Brezhnev's personal cardiologist. Thus, the "message" was directly conveyed to the Soviet citizenry by Dr. Chazov in a nationwide television broadcast during Soviet prime time.

These facts are important, in view of the widespread conviction in the West that the anti-nuclear movement, and its propaganda, is essentially a Western phenomenon. It is important, for example, to recognize that the "Appeal by West German Physicians to Physicians in the USA," which asserts that "the deployment of these modernized weapons and the new military strategy for a limited and winnable nuclear war means an enormous increase in the danger of war," was matched by an "Appeal by West German Physicians to Physicians in the Soviet Union" to "petition your government to call a halt to the deployment of SS-20 missiles."

The entire medical profession is involved in this movement and not least the psychiatrists. The World Association for Social Psychiatry, representing essentially all the American psychiatric bodies as well as the International Psychoanalytic Association, in adopting a Resolution Against Nuclear War, has called on all

(From left) Bernard Feld, Morton Grodzins, Leo Szilard, Harrison Brown, Sir Robert Watson-Watt at Pugwash, Lac Beauport, Quebec, Canada, 1958.

The Russell-Einstein Manifesto was a proclamation by the world scientific community to end the East-West confrontation.

the members of the profession to

"exert every effort to avert a nuclear catastrophe:

- By educational campaigns as to its calamitous and irreversible consequences.
- And by utilizing other individual and organizational means to preserve humanity in its most critical time in history."

There is as well obviously considerable seething among "intellectuals" behind the "iron curtain." For example, the statement by a group of East European scientists—commemorating the thirty-fifth anniversary of the founding of the Emergency Committee of Atomic Scientists in the United States to combat the dangers of nuclear war—weighed the dangers of growing nuclear deployments in Europe and proposed that "all the governments of the world should [unanimously] declare that they will never be the first to employ nuclear weapons [and] that they will never use nuclear weapons against a state which does not possess nuclear weapons of its own and on whose territory no nuclear weapons are stationed." This appeal, in what seems to be a most even-handed approach, asks both NATO and the Warsaw Pact nations to "declare a moratorium in the stationing of new rocket weapons and in the updating of nuclear weapons including the neutron bomb, to carry on negotiations for the reduction of nuclear weapons with the highest priority until their final destruction and thus give substance to Article VI of the treaty against the proliferation of nuclear weapons."

The scientists are not alone. Religious leaders are now coming to the fore in the United States as well as in Europe, stressing the moral and ethical issues involved in the preparation for nuclear war and in *any* use—defensive as well as offensive—of nuclear weapons. For example, the Pontifical Academy of Sciences, under the Chairmanship of Professor Carlos Chagas of Brazil, is a most distinguished international group whose most recent declaration is published in this issue.

The Pope himself, in a message to Reagan and Brezhnev, stresses that "our generation has the moral duty to spare no effort to exorcise the specter of nuclear war and to banish the temptation to yield to the idea that it is something inevitable."

The World Council of Churches, as a consequence of a Public Hearing on Nuclear Weapons and Disarmament, held in Amsterdam in November 1981, issued a pamphlet, *Before It's Too Late*, which calls for a complete test ban and a stop in production of nuclear weapons.

From the other side of the "curtain" we have the 1981 statement of the Lutheran Churches in the German Democratic Republic, in which East German religious leaders join with the synod of the Netherlands Reform Church in denouncing "a strategy of 'limited' nuclear wars . . . and to repeat our 'No' . . . and clearly declare that this 'No' without restriction applies also to the possession of such weapons."

Europeans, for the most part, feel themselves somewhat outside the nuclear decision-making process and therefore confine their criticisms and proposals to rather general considerations of principle. Americans, on the other hand, still feel (justifiably or not) in the political middle of things, and proposals originating in the United States accordingly tend to be rather more specific. Witness the Congressional resolution sponsored by the Federation of American Scientists which, with admirable brevity, suggests that "the United States should not base its policies or its weapons programs on the belief that the United States can limit, survive, or win a nuclear war." Acceptance of this principle—unhappily still eluding military leaders on both sides of the nuclear confrontation—would indeed go a long way toward dissipating the fear of nuclear annihilation that beclouds the Earth. I suspect that universal—or at least bilateral Soviet-American—adherence to this principle is needed before we can hope for the initiation of those drastic measures of nuclear disarmament that are a prerequisite to the establishment of meaningful nuclear detente.

Concrete programs to move in this direction have been formulated by a number of groups. The Union of Concerned Scientists, for example, proposes four specific steps:

- The Comprehensive Test Ban;
- Limitations on flight testing of nuclear missile systems;
- Substantial and verifiable reductions in present U.S. and Soviet nuclear arsenals;
- A coordinated and intensive program to stop the spread of nuclear weapons and to reduce the inventories of other nuclear states.

A proposal sponsored by the American Friends Service Committee advocates substantial U.S. military budget cuts; a U.S. no-first-use declaration; a ban on flight testing of new delivery systems; a cutoff on production of fissionable material for bombs; a moratorium on underground nuclear explosions pending agreement on a universal test ban; suspension of new deployments in Europe; and a two-to-three-year moratorium on all new weapons and weapons systems pending agreement on their substantial reduction. A similar program has also been set out by the Council for a Livable World.

Clearly, there is no dearth of ideas on how we may start to move away from the dangerous brink to which nuclear policies on both sides have brought us. What is lacking is the necessary sense of urgency in official circles. ☐

ROBERT GOMER

Forty years ago

Across the street from the building where I work stands a massive bronze object. Looking out of my office window I can often see it gleaming in the sun, its smooth top reflecting the light from that distant chain reaction, distilling out the amber gold. Tourists in buses come by to look at and photograph it. It is shaped like a skull, or a helmet, or a mushroom cloud; no one has quite deciphered the exact meaning of Henry Moore's sculpture commemorating the birth of the nuclear age. It is located exactly on the spot where Enrico Fermi and his colleagues ushered in that age. The birth took place in a former squash court under the West Stands of what was then the football field of the University of Chicago; it went very smoothly, with a break for lunch, and was celebrated by its attendants with a modest glass of Chianti.

Henry Moore's sculpture "Nuclear Energy" at the site of the first chain reaction.

If no one has completely understood Moore's sculpture, it is fair to say that no one has fully fathomed the consequences of the event it commemorates. For the first time in human history it has enabled mankind to destroy itself utterly, finally, irrevocably. For the better part of the four decades since that first man-made self-sustaining nuclear chain reaction, the *Bulletin* has tried to alert the world to that fact, from which there is no turning away. Amazingly enough, the message seems only now to be sinking in. While the existence of nuclear weapons has probably prevented direct conflict between the superpowers so far, the world has not become a safer place because of them. On the contrary, the arms race threatens to engulf us all, and our highest priority must continue to be the search for a way out of this terrible dilemma.

The event on December 2, 1942 was not isolated from previous research. All science is to a very real extent interlinked, and picking out only some high-water marks can be misleading. Some do stand out, however. Several articles in this issue, for instance, comment on the fact that 1982 is also the fiftieth anniversary of Chadwick's discovery of the neutron. At that time, most physics was considered the realm of cloistered scientists whose work and persons had little to do with the "real" world of business, politics or war. World War II and the almost unbelievably swift evolution of technology based on physics, chemistry, and, most recently, biology since then have of course changed all that. Nuclear weapons and nuclear energy are only two of the most visible and dramatic consequences of this revolution. But there are infinitely many more. Let me cite a few examples, picked almost at random: Instead of educating and uplifting us, television has made us a nation of semi-illiterates and daily floods us with stultifying junk. Ad-

Neither physics nor any other science, but only decency, humanity and good sense can lead us safely past the dangers.

vances in agriculture have made it possible to grow food more cheaply and with higher yields than dreamed possible a hundred or even fifty years ago, but they have contributed to poisoning soil and water and have worsened not helped the economic problems of farmers. Mechanization may make the terrible monotony of the assembly line a thing of the past, but threatens the livelihood of millions of workers.

Through all these ironies runs a common thread, nowhere more dramatically visible than in the consequences of what Fermi and his colleagues accomplished on December 2, 1942: The pace of science and technology has far outstripped man's ability to cope with the results. Far from fructifying the earth and lightening man's burden, science may have put us on the verge of extinction, not because technology is inherently evil, but because most of us are too primitive, too shortsighted, too selfish to grasp its implications, to recognize the dangers, and to respond to them adequately and in time.

When the gods send storms, there is always a tendency to blame the priests, and so it has been with scientists. Their image in Western societies has undergone some curious transformations; once considered harmless cranks, they were next revered as the bringers of thunder, and are now being execrated for the hail. None of these attitudes makes much sense. For instance, it is absurd to blame the early atomic scientists of the Manhattan Project for having built the bomb, working as they were in the belief that Germany was ahead of us. Despite the fact that the scientists had virtually no say in how the bomb was to be used, once they had turned it over to the military, it is to the credit of many that they tried to prevent its use on civilians.

Blaming scientists for the consequences of technology is looking for scapegoats, but that does not mean that scientists have no responsibilities toward society. On the contrary, they often have the knowledge if not always the wisdom to see what the consequences of technology may be, and it is their duty to alert society at large to that knowledge. It is precisely out of this sense of responsibility that the *Bulletin* was born thirty-seven years ago, and this issue also discusses efforts of scientists immediately after the war to control what they had wrought.

It is always dangerous to be possessed of special knowledge, because with that knowledge need not come either wisdom or humility. Over the last twenty years there has been serious talk of the "technological fix": If science has made a mess of things, more science and considerably more technology can get us out of the mess. Many scientists and engineers, many economists, and many politicians have subscribed and still do subscribe to this view. In my opinion absolutely nothing could be a grosser misunderstanding of our problems. Today, for many millions of people, the arms race is an obvious *reductio ad absurdum* of that attitude. More weapons are not the cure for the fix weapons have put us in. But even in the effort to reduce the arms race, technology has only a very limited part to play. The essence of this problem is political, not technical. Until all of us, scientists included, see that the solution to social, political, and economic problems lies in the realm of social, economic, and international policy, not in technological fixes, we will not make much progress in coping with them.

Science and scientists have much to contribute toward solutions of the multiple ills which face the entire world today—the ever present threat of annihilation, hunger, oppression, overpopulation—but they and those they try to help must keep in mind that technology is only a means to various ends. It must be society that decides on the ends, and not technology.

How are we to utilize the knowledge and judgment of scientists and engineers when controversy rages among scientists themselves, for instance over the issue of nuclear power? There are no simple answers. Human beings, including scientists, are subjective, and their judgments are colored by their prejudices. The best we can hope for is a clearer statement of the issues, and if possible agreement on facts. The availability of experts is no substitute for an intelligent and informed body politic.

Nowhere is this more true than in the debate over the arms race. Fortunately, an ever increasing number of people are beginning to take interest in the facts and are beginning to draw their own conclusions. Fortunately, as Goethe has God say in *Faust*, "Ein guter Mensch in seinem dunklen Drange ist sich des rechten Weges wohl bewusst"—a good man in his dark gropings is well aware of the right—that is, he can trust his instincts. Ultimately that is our only hope. Neither physics nor any other science, but only decency, humanity, and good sense can lead us safely past the dangers unleashed by the beast that came slouching toward Stagg Field, waiting to be born, on that fateful December day, forty years ago. □

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Fifty years ago James Chadwick discovered the neutron. This was the culmination of more than a decade of frenetic activity in the small community of nuclear physicists. In the following article, two scientists reflect on their experiences with Chadwick at the Cavendish Laboratory.

MAURICE GOLDHABER

With Chadwick at the Cavendish

Why should one be interested in personal reminiscences? Older people may have nostalgic reasons, wanting to hear again about the "good old days." The younger generation might learn that some things were done differently and perhaps better long ago. And, though it may be difficult to bring back some of the old ways of doing business, we should try to do so when we are convinced that they were better. Also, young physicists today hear so much about future physics that they may find that past physics is sometimes more exciting to listen to for a change.

Historians of science may want to learn of some raw material for a more definitive history. They must keep in mind, though, that memory is selective and protective of the ego. There are usually contradictions among the reminiscences of different scientists which the historians must sort out to get, if not the "ultimate historical truth," at least a more consistent picture.

In personal reminiscences we can mention ideas and experiments which did not succeed and often did not lead to publications. We can tell of near misses because one was either too "clever" or had too insensitive a detector or too weak a source. And we can talk of motivations and other relevant information not usually mentioned in journals. It might benefit historians of science if authors would send, along with the papers they submit to scientific publications, a sealed note giving their motivations, how their ideas arose, who affected their thinking and so on—not to be made available for, say, 50 years.

Well before Chadwick discovered the neutron he had already distinguished himself in nuclear physics. Among his most important contributions was the discovery he made when he was only 23 years old—that the beta spectrum is continuous, rather than a collection of discrete lines as had been believed. He was at that time working in Geiger's laboratory in Berlin. It was 1914, and when World War I started the Germans interned him in a civilian camp where he stayed for four years, amusing himself by doing experiments on radioactvity. He also interested another internee, C. D. Ellis (later famous for his work on beta rays), in the subject. Together they investigated a German toothpaste, advertised as radioactive, and for a while they thought they had discovered a new radioactive series. The bad food at the Camp ruined Chadwick's digestion, and he suffered from this for the rest of his life.

Chadwick was 20 years older than I, and 20 years younger than Rutherford. Twenty years is a long time in the development of physics, covering something like five successive generations of research students, with each generation confronting physics in a different state of development.

I came to Cambridge 49 years ago and thus missed the excitement at the Cavendish when the neutron was discovered. At that time I was still a student at Berlin University, where I had taken a course in nuclear physics given by Lise Meitner. When Chadwick's discovery became known, Meitner reported on it in an extremely well attended colloquium. She was so excited that she misspoke herself and talked of the neutron hitting a "brass nucleus."

Early in 1933 it became clear that I had to interrupt my studies and leave Berlin. I had gone as far as to talk with Schroedinger about a possible theoretical thesis, but soon we both decided that the time had come to leave Germany. Of the physicists to whom I wrote, Rutherford was the first to answer. He accepted me at the Cavendish, and I came up to Cambridge to find out more details about the life of a student here. I had heard that the cheapest way to live as a student was to become a member of Fitzwilliam House, but when I told Chadwick about this he made the somewhat cryptic remark: "If I were you, I would join a college. They do things for you." Then I met David Schoenberg and asked him to tell me about some good colleges. Pointing to Trinity Street, he said, "In this direction you will find Trinity, St. John's, and Magdalene."

At Trinity I was told they were already full up. At St. John's I heard that they would let me know in six weeks. At Magdalene the Senior Tutor said, "Ah, you are a refugee; I suppose we ought to have one," adding, "I suppose you have no money. We'd better give you a hundred pounds," which was about half of what a research student needed then for a year. Chadwick had given me good advice. It seems hard to believe now that there was so little red tape in those days.

In the spring of 1934 I worked on the role of nuclear spin in the disintegrations studied by Cockcroft and Wal-

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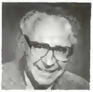

ton and also wrote a note on what is now known as "delayed neutrons." I went to see Chadwick twice to discuss my work with him. On one of these occasions I found the courage to suggest the photodisintegration of the deuteron (then called the diplon at the Cavendish). The photodisintegration of the deuteron seemed a way to obtain a precise mass for the neutron, a possibility that interested Chadwick. At that time the Joliots claimed a rather high mass for the neutron; Ernest Lawrence argued for a small one; and Chadwick's value was in between, though lighter than the proton.

About six weeks later Chadwick said to me: "Were you the one who suggested the photodisintegration? Well, it works; it worked last night. Would you like to work with me on this?" I immediately said yes, and then got Fowler's blessing. I assume that Chadwick discussed this change with Rutherford at one of their daily meetings. We worked intensely for about two months, and found that the neutron was definitely heavier than the proton, even heavier than the hydrogen atom. I remember being shocked by the realization that an "elementary particle" like the neutron might decay by beta-emission. I crudely estimated its half-life as about 30 minutes.

When we started writing a note about our work to *Nature*, sometime in July 1934, we noticed an interesting conflict between our results for the photodisintegration cross section ($D + \gamma \rightarrow p + n$) and observations made by D. E. Lea earlier. He had bombarded paraffin with fast neutrons and discovered gamma-emission, which he ascribed to the inverse reaction $p + n \rightarrow D + \gamma$.

We realized that our measured photodisintegration cross-section would lead—regardless of specific assumptions—to a prediction for the cross-section for the inverse reaction smaller by many orders of magnitude than that reported by Lea. Thus, the effect Lea observed could not be due to the *primary* neutrons from his source. Early drafts of our paper—which I still have in Chadwick's and my handwriting—contained a speculative interpretation in which we assumed that the fast neutrons are first slowed down and then captured as *slow* neutrons, but Chadwick said later, "Let us not speculate." Under Rutherford's influence speculation was somewhat frowned upon at the Cavendish. And by that time I had absorbed enough of the Cavendish spirit to agree. We merely pointed out the puzzle we had encountered, ending with the bland statement: "It therefore seems very difficult to explain the observations of Lea as due to the capture of neutrons by protons, for this effect should be extremely small. A satisfactory explanation is not easy to find and further experiments seem desirable."

A few months later, when Fermi and his collaborators discovered slow neutrons, the paradox we had noticed was resolved. Rutherford sought me out, uncharacteristically agitated about the fact that this discovery had gotten away from the Cavendish. A psycho-historian of science might suspect that it was he who had talked Chadwick out of our speculation.

After the discovery of slow neutrons, we were mentally prepared to exploit it. We started by searching for the disintegration ${}^6Li + n \rightarrow {}^4He + {}^3H$ and found a good yield. This reaction is sometimes called a fusion reaction and sometimes a fission reaction. We then bombarded many more elements and found reactions in boron and nitrogen. When we exposed uranium to slow neutrons, we hoped to find long-range alpha-particles. We therefore "cleverly" covered the uranium target with aluminum foil thick enough to stop the naturally emitted alpha-particles—that is, also thick enough to stop fission particles!

The lithium, boron and nitrogen reactions, which were easily found, have led to many applications, and, like most discoveries, they can be used for good or ill. Reactors have made it possible to use the lithium reaction to make large quantities of hydrogen-3, important as a tracer and for fusion reactions; its daughter product, helium-3, is important in low temperature research (a "cold war" surplus). The boron reaction is important as a neutron detector, especially in the boron trifluoride counter, one of which we built. This reaction also holds some promise in cancer treatment. The nitrogen reaction leads to the important tracer carbon-14, made in reactors as well as in nature by cosmic-ray neutrons in the atmosphere.

At the same time that Chadwick was packing up to leave for Liverpool in the summer of 1935, we were preparing a fuller account of our photodisintegration investigations for the *Proceedings of the Royal Society*. When I visited him at home where he re-wrote our paper in his beautiful handwriting (secretaries were still a rarity), he told me that he used a simple principle to decide what he would take along: If he would not need it for the next two weeks, he would leave it behind. He came across the paraffin wax he had used to knock out protons when he discovered the neutron and said to me: "Do you want it? I don't need it." I was glad to have this historic memento, which is now on loan to the Science Museum in Kensington.

The Cavendish has the distinction of having started the electronic age in 1897 with the discovery of the electron by J. J. Thomson. And what one might call the "neutronic age" started there in 1932 with Chadwick's discovery of the neutron. The electronic age extends our senses; the neutronic age may still bring us to our senses. ☐

MARK OLIPHANT

The beginning: Chadwick and the neutron

Fifty years ago James Chadwick discovered the neutron. From this inspired observation arose a revolution in warfare, a new source of energy, and fundamental changes in both the direction and the techniques of the physical sciences, and some aspects of the biological sciences. It is unfortunate that the names of those who made and used weapons of war have become household words, while those who used the neutron for peaceful purposes, like its discoverer, have never received the acclaim they deserve.

Born near Macclesfield, not far from Liverpool, of middle class parents, Chadwick spent his boyhood in that region. He was educated in state schools where a special aptitude for mathematics and science soon became apparent. He decided to become a mathematician, but, having won a scholarship to Manchester University, his experience of the entrance interviews led him to change his mind, and do physics as his principal subject. This choice was confirmed when, in his second year, he attended the stimulating lectures given by physics professor Ernest Rutherford, that great New Zealand-born scientist who inspired a whole generation of physical scientists. Chadwick received a First Class Honors Degree, his external examiner being J. J. Thomson, discoverer of the electron. Then, as a research student, he came to know many of the leading physicists of the period. Among them were Geiger and Marsden, who made the observations of the scattering of alpha-particles from radium, upon which Rutherford based his concept of the atomic nucleus. There was Moseley, whom Chadwick detested, but whose brilliant work with X-rays established the identity of atomic number and positive nuclear charge, and many others from Europe and elsewhere.

In 1912 Niels Bohr spent nearly six months with Rutherford, during which he became fascinated with the structure of the atom as revealed by Rutherford's work. Chadwick was much impressed by Bohr, by his intuitive grasp of and interest in all science, and by his kind and generous nature. They became lifelong friends.

James Chadwick

Chadwick earned his Master of Science degree in 1913, and through Rutherford's recommendation was awarded an Exhibition of 1851 Senior Research Studentship, which had to be pursued away from the institution from which he had obtained his degree. Chadwick's interest in radioactivity could be satisfied in few laboratories at the time, and no other one in England.

He chose to join Johannes Geiger, whom he already knew and admired, at the Reichsanstalt, in Berlin, where he learned much of German physics and physicists. Sir Harrie Massey, in his excellent "Biographical Memoir" for the Royal Society, has described that rewarding year in detail, including the story of Chadwick's arrest and internment in a camp at Ruhleben. He was well treated by his German friends, and was even able, with their help, to do some experimental work with crude equipment. There also, he met Charles Ellis, an engineer officer of the British Army, who was later to become his colleague in Cambridge. He returned to England in 1918 and rejoined Rutherford as a member of the physics staff in Manchester, where they worked together on the nuclear transformations produced by energetic alpha-particles from the radioactive products of the spontaneous disintegration of radium. Chadwick told me that he had great respect for Rutherford, but was also awed by him.

In 1919, when Rutherford became Cavendish Professor of Physics in Cambridge, succeeding his own teacher, J. J. Thomson, he took Chadwick with him. So began a partnership which was to last for 16 years, which would make the Cavendish Laboratory the Mecca for physicists, and which would produce many of the foremost physicists of my generation.

In consultation with Chadwick, Rutherford decided that their research in the Cavendish should be the study of the atomic nucleus, a decision

about which there was not universal agreement.

As Rutherford's lieutenant, Chadwick became responsible for the direct supervision of all research work in the Cavendish, Rutherford himself having so much to do that he had little time to confer with research students. In 1927, when I joined the Laboratory, I found that all students were overawed at first contact with Chadwick who, in fact, was a most sympathetic and kindly man.

Upon arrival in Cambridge, I was directed past a hall filled with bicycles, and along a passage with bare wooden floors and dingy walls, to Rutherford's office. I wondered how any science was done in such decrepit surroundings. This feeling vanished when Rutherford's booming voice invited me into his smoke-filled, untidy small study. After discussing what I wanted to do, and approving it, he said: "Now go 'round and meet the boys. At this time of the day you will probably find J. J. in the Garage, and beyond there Aston may be at work. Chadwick will be upstairs, I think, preparing some radioactive sources in the Tower. Make yourself known to them all."

Dazed, I sought the Garage, which proved to be an underground laboratory of considerable size. I felt in no state to meet such great men as J. J. Thomson or Aston, and fortunately neither was in. Seeking Chadwick, I met a tall, handsome man in a passage, who stopped and said: "I'm Blackett. Who are you?" When I told him he grunted and walked on. At last I found Chadwick and introduced myself. The lean man in a neat, dark suit looked at me over the top of his glasses, with an intense stare which I came to know well over the next 47 years. He was silent for an embarrassing few moments; then his face broke into a rare smile as he said: "Oh yes. I've been expecting you. I'll show you where you are to work." He led me to a large room near Rutherford's office, explaining that one-quarter of its area would be mine. With a limp handshake, characteristic of both Rutherford and Chadwick, he left me. Such was my first meeting with a man who was to become a close friend, but who then scared me stiff.

Ernest Rutherford

At that time very primitive hand-operated vacuum pumps were used in the Cavendish. A few months after I began work, I became very tired of the necessity to turn the handle for long periods every hour. When Chadwick came to visit me he asked whether there was anything I needed. I said that a motor-driven vacuum pump, called a "Hyvac," would help enormously. With that daunting stare he said: "So you want a Hyvac, do you? Well! You just can't have one." Feeling rather depressed, I went off to lunch. When I returned there was a Hyvac on my bench! I learned then that Chadwick's heart belied that stare. But with 40 research workers in the Cavendish, the miserable £2,000 a year allotted for equipment had to be used as fairly as possible. Chadwick did his best for all of us, but he could not perform miracles. Rutherford believed in simple ways of doing research, so those were the days of coffee tins, string and sealing wax.

At first, I shared the room with two other Australians. J. K. Roberts, a more senior man from Melbourne, had worked with Rutherford and Chadwick in the early search for the neutron. He had come to the Cavendish to work on the exchange of heat between a hot wire and a gas, bringing with him a chronometer watch, which had to be handled very carefully. His first act each day was to place it in the drawer of his bench. Chadwick remarked to me that Roberts paid too high a price in order to know the time with an accuracy of a few seconds, which was totally unnecessary for the excellent work he was doing. The other was Arnot, from Sydney, who began experiments on the scattering of very slow electrons.

Edward Bullard, who was to become the initiator and most noted British worker in both experimental and theoretical studies of the Earth (geophysics), took over from Arnot. Wishing to use a large coil of wire to produce a magnetic field, he wanted to know how much wire was on the solenoid. So, rigging up a suspended beam as a crude scale, he tied the coil to one end, and sat Harrie Massey in a loop of rope which he slid along the other arm. Thus, he determined the weight in "Masseys," a known unit, and was able to calculate the number of turns of wire in the coil. Chadwick's grin of appreciation of Bullard's ingenuity, reminiscent of the techniques used by him and Ellis in the internment camp, burst into a chuckle—an almost unheard of event. Bullard came from a brewing family, which produced Bullard's Norwich Ales. He amused Chadwick greatly on another occasion when he asked innocently of the absolute alcohol he was

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using to clean his apparatus: "Do alcohol and water mix?"

Chadwick examined all papers published from the Cavendish. He was a severe critic of English usage, and seemed to carry in his head the whole of Roget's *Thesaurus of English Words and Phrases*. He was equally censorious of poor scientific argument, or of sloppy conclusions from inadequate experimental results, not even sparing Rutherford's own papers. Despite his inspiration and continuous help, Chadwick did not claim any credit for his part in much of the most important work done in the Laboratory. Under his guidance many young men achieved reputations not wholly deserved, as was shown when they moved away from his tutelage.

In 1920, Rutherford gave the Bakerian Lecture of the Royal Society. Speaking of the difficulty of explaining the results of experiments he had done with Chadwick and others, he said of one possibility:

"It involves the idea of the possible existence of an atom of mass 1, which has zero nucleus charge. Such an atomic structure seems by no means impossible . . . on present views, the neutral hydrogen atom is regarded as a nucleus of unit charged with an electron at a distance. . . . Under some conditions, however, it may be possible for an electron to combine much more closely with the H nucleus, forming a kind of neutral doublet. Such an atom would have very novel properties. Its external (electric) field would be practically zero, except very close to the nucleus, and in consequence it should be able to move freely through matter . . . it should enter readily the structure of atoms, and may either unite with the nucleus or be disintegrated by its intense field. . . . The existence of such atoms seems almost necessary to explain the building up of the nuclei of heavy elements; for . . . it is difficult to see how any positively charged particle can reach the nucleus of a heavy atom against its intense repulsive field."

Rutherford and Chadwick themselves, and with the help of research students, carried out many experiments in search of this elusive particle, but without success. Chadwick, in a lecture at Cornell University, wrote of the search for the neutron which followed Rutherford's remarkable intuition. With characteristic modesty, Chadwick does not make it clear that while Rutherford never lost his deep interest, it was Chadwick's persistence in following up every possible indication that finally brought success. He said of Rutherford:

"He had not abandoned the idea, and he had completely converted me. From time to time in the course of the following years, sometimes together, sometimes myself alone, we made experiments to find evidence of the neutron, both its formation and its emission from atomic nuclei. . . . We looked for faint scintillations due to a radiation undeflected by a magnetic field."

Chadwick continued: "The case of beryllium was interesting for . . . it did not emit protons under alpha-particle bombardment. . . . This matter intrigued me on and off for some years . . . and for a short and exciting time we thought we had found some evidence of the neutron. But somehow the evidence faded away."

Then, one morning, Chadwick wrote that he had read:

"the communication of the Curie-Joliots in the *Comptes Rendus*, in which they reported a still more surprising property of the radiation from beryllium, that of ejecting protons from matter containing hydrogen, a most startling property. . . . A little later that morning I told Rutherford . . . about the Curie-Joliot observation. . . . I saw his growing amazement; and finally he burst out, 'I don't believe it.' Such an impatient remark was utterly out of character, and in all my long association with him I recall no similar occasion. I mention it to emphasize the electrifying effect of the Curie-Joliot report. Of course, Rutherford agreed that one must believe the observations; the explanation was quite another matter.

It so happened that I was just ready to begin experiment, for I had prepared a beautiful source of polonium. . . . I started with an open mind, though naturally my thoughts were on the neutron. . . . I was convinced that there was something quite new as well as strange. A few days of strenuous work were sufficient to show that these strange effects were due to a neutral particle and to enable me to measure its mass: the neutron postulated by Rutherford in 1920 had at last revealed itself."

Thus the neutron was discovered as a result of a persistent search by Chadwick, and not by accident as were radioactivity and X-rays. Chadwick felt intuitively that it must exist and never gave up the chase.

Soon after the discovery, Chadwick spoke to the small club founded by Peter Kapitza to discuss hot news in physics, an occasion which I remember vividly. Kapitza had taken him to dine in Trinity beforehand, and he was in a very relaxed mood. His talk was extremely lucid and convincing, and the ovation he received from the select audience was spontaneous and warm. All enjoyed the story of a long quest, carried through with persist-

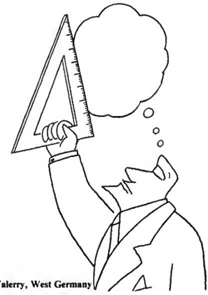
ence and vision, and they rejoiced in the success of a colleague. Chadwick's meticulous recognition of the parts played by others in pointing the way was a lesson to us all.

I shall not dwell on Chadwick's move to Liverpool in 1935, his building of a cyclotron, or of the work done with it. Rather, I wish to give a personal account of his contribution to atomic energy—the major practical result of his discovery of the neutron.

Experiments on the absorption of neutrons in various substances, and the nuclear transformations they produced, had been carried out in Rome by Fermi, and in Paris by the Curie-Joliots. There was evidence that when neutrons penetrate into the nuclei of uranium atoms, chemical elements heavier than uranium of mass 238, the heaviest atom occurring in nature, might be produced. Otto Hahn, a German chemist who had spent some time with Rutherford in Montreal early in the 1900s, discovered with his colleague Fritz Strassman that when uranium absorbed a neutron it sometimes broke into two lighter atoms, like barium and iron—the fission process. Otto Frisch and Lise Meitner, in Copenhagen, found that enormous energy was released in this process, while Joliot and his colleagues in Paris showed that more than one neutron was emitted during fission.

There was the possibility that these fission neutrons could sustain a nuclear chain reaction in uranium, and hence an explosion of unprecedented power. Chadwick was consulted officially about this possibility and, having read the theoretical paper by Niels Bohr and John Wheeler, he cautiously said that it might be possible, but that it was necessary to know more about the probability of fission as a function of neutron energy. He set to work to measure these probabilities, called cross-sections, using his recently completed cyclotron.

G.P. Thomson in London and P.B. Moon in my laboratory had been attempting to produce a chain reaction by slowing down the fission neutrons in water, but without success. Then Rudolf Peierls and Frisch, my colleagues in Birmingham, whose German origins precluded their contribution to our top-secret work on radar, came to me in 1940 with calculations they had made, based on the Bohr-Wheeler theory. They convinced me that if the less prevalent isotope of uranium could be separated in a pure form, it might be possible to make a weapon using directly the fast fission neutrons; that the mass required would be only of the order of a kilogram; and that the chain reaction should build up very rapidly, before the material expanded appreciably. I took this report to G.P. Thomson, chairman of the so-called Maud Committee, which had been established to examine the possible military application of nuclear fission in the war. The Committee generally was electrified by the possibility, but Chadwick, who was also a member, was embarrassed, confessing that he had reached similar conclusions, but did not feel justified in reporting them until more was known about the neutron cross-sections from experiments Peierls and Frisch had used calculated values. However, this confirmatory evidence led the Committee to pay great attention to the development of techniques for the separation of uranium-235 from the far more abundant isotope 238. Chadwick coordinated all these investigations, and his team in Liverpool was able to show that the fission cross-section of 235 was at least four times greater than had been indicated from U.S. experiments. Through the Secret Service it was learned that Hitler's Germany was interested in a possible nuclear weapon, and we knew that there were German scientists who were capable of reaching the same conclusions as we had in Britain.


Valerry, West Germany

The minutes and reports of the Maud Committee had been sent to Lyman Briggs, chairman of a similar committee in the United States, and we were puzzled to receive virtually no comment. In 1941 I went to the United States on radar business and was asked to make discreet enquiries about this situation. I called on Briggs in Washington, only to find that this inarticulate and unimpressive man had put the reports in his safe and had not shown them to members of his Committee. Amazed and distressed, I reported the situation to Vannevar Bush and James B. Conant, who were responsible for the application of science to a war in which America was rapidly becoming involved, although officially still neutral. Not satisfied with their response, I raised the question with Ernest Lawrence, inventor of the cyclotron and the most dynamic member of the Briggs Committee. He was deeply interested, and I am sure that it was his influence which led to a very rapid escalation of effort in America.

After some breakdown in communication between Britain and the United States on these matters—largely because of suspicions on the

The nuclear weapon, Chadwick felt, could not be left in American hands alone, and it was Britain's duty to possess the weapon as soon as possible.

American side about the possible Communist sympathies of some of the British personnel—Churchill and Roosevelt signed the Quebec Agreement on Anglo-American collaboration in August 1943. Chadwick was readily accepted by the Americans as the leader of the British Mission in Washington.

In September, I accompanied Chadwick to the United States to work out the details of cooperation. He had not been in America previously, and had been told by the American-born Lady Astor that he must visit Harvey's Oyster Bar in New York. Soon after arriving we took a taxi to the recommended restaurant, where Chadwick ordered, and ate, a dozen oysters. Later that night he became terribly ill, and in the morning was still obviously suffering. But he insisted that we fly to Washington, where we were to report to the British Embassy, after which Chadwick went alone to see General George Marshall, and then General Leslie Groves, who had been appointed to administer the U.S. atomic energy program. I was very anxious as I saw an extremely sick but determined man into his car.

Perhaps it was because of the special effort he had to make that both visits were most successful. In particular, he and Groves formed immediately a relationship of trust and understanding, which ripened over the years into affection. Groves was disliked by many of the U.S. scientists, and he referred to them as "the long hairs." But I shared Chadwick's admiration for so good an administrator, never afraid to make a decision, even though it involved a billion dollars. This close relationship between Chadwick and Groves was to be of immense importance for the subsequent British work in nuclear energy.

In November 1943, most of the British workers moved to the United States, as it was clear that such a large and expensive project was beyond the wartime resources of the United Kingdom. Although attached primarily to the team led by Ernest Lawrence, which was developing the electromagnetic technique for separating uranium isotopes, I wandered around other parts of the United States and also the British-Canadian projects. I also spent much time in Washington, and served as Chadwick's courier, flying across the Atlantic by bomber. Chadwick had been able to get Bohr smuggled out of Denmark via Norway, and Bohr was often in Washington also, particularly after success became almost certain. Bohr was anxious that the new weapon should be used to bring peace to the Earth, rather than to kill hundreds of thousands of people. Chadwick had such affection and high regard for him that he tolerated his lobbying and attempts to see the President, even though these were politically embarrassing.

An old-fashioned British patriot, Chadwick was wholly devoted to all that the United Kingdom represented. He admired and liked many Americans, but he did not trust their political system nor its "way of life." He felt strongly that the nuclear weapon could not be left in American hands alone, and that it was Britain's duty to possess the weapon as soon as possible, and thereby to have at least one sane and experienced nation with some say in its development and use. In 1945, just before the first test at Alamogordo, he sent me back to England to try to get things moving there. He wished to maintain close cooperation with the U.S. effort, but was equally determined that the British project should not be subservient to it.

Chadwick returned to England and his professional chair in Liverpool in 1946, completely exhausted physically and mentally. But he continued to have great influence upon both government and scientific policy and activities. His team in Liverpool built a successful synchro-cyclotron, and he took a deep interest in the plan to establish a major European international research centre, CERN, in Switzerland, although he hated to give up British independence.

By the late 1940s Chadwick, whose health remained precarious, was not happy about the direction in which nuclear physics was developing, though he remained interested in high energy particle physics till it became too complex for him to follow it fully. He told me that all the excitement had gone from the subject to which he had devoted his life, and likened nuclear structure investigations to what had happened to spectroscopy after Bohr had so successfully applied quantum theory to atomic structure. But he remained deeply interested in physics generally.

In 1948 Chadwick was invited to become Master of Caius College, his own College in Cambridge, to which he felt that he owed a great debt of gratitude. But during his 10 years as Master he missed greatly being able to work in a laboratory. He did much for his College, but his life as a scientist was over. In 1958 he resigned, and went to live in a small house in North Wales. He and Lady Chadwick were able to mix again with their friends in and near Liverpool. But his health declined further, and in 1969 the Chadwicks returned to Cambridge, primarily to be near their twin daughters. He died, worn out and somewhat disillusioned, in 1974.

In his foreword to my little book on Rutherford's Cambridge days, Chadwick quoted at the end from Thomas Hardy's *The Woodlanders*, words that are as true of him as they were of Rutherford: "If I ever forget your name let me forget home and heaven . . . I can never forget 'ee; for you was a good man, who did good things." □

PHILIP MORRISON

The nuclear plateau

Like a good map, anniversaries set over perceived reality a rather arbitrary grid of scale. This year 1982 offers by chance a very instructive lattice of decades; I believe it is a hopeful one.

In the year 1932, James Chadwick published in *Nature* clear evidence for the neutron. Nuclear physics needed that discovery badly. The concept was not at all new; Rutherford had sought it as early as 1920. The day after Chadwick, across the sea, Harold Urey submitted his own finding of the deuterium atom, another prescient old expectation of Rutherford's.

The neutron seemed part of the history of physics, but hardly of Everyman. A few, like Leo Szilard, divined the connection between neutrons and the long-impossible dream of large-scale nuclear energy release. Our anniversary of course is really that of the fateful day of December 2, 1942, when the fission chain reaction first became self-perpetuating under Enrico Fermi's guidance. Nuclear energy could no longer be turned loose only particle by particle, but now by the kilogram. Molecule implied mole, and a mole of suddenly-fissioned uranium is almost 5,000 tons of TNT, or under careful control the power of a big urban power plant for the better part of a shift.

But the anniversaries continue surprisingly. On the first day of November 1952 the islet of Elugelab in the atoll necklace of Eniwetok was caused to disappear, leaving only far-flung dust and a sea-filled hole a mile across at the edge of the lagoon. The neutron had been put to work as trigger, then, the deuterium of Urey was consumed by the hundred kilogram in the hot plasma. The applied astrophysics of Edward Teller and Stanislaw M. Ulam had enabled the first thermonuclear reaction of unlimited potential.

Twenty years carried Chadwick's Cavendish Laboratory experiment first into the fission chain reaction, and then to the thermonuclear bomb. Thermonuclear war had become the fear of the world and the final threat of the diplomat, in just 20 years. No physicist, at least, can overlook the hint. What happened in the next three decades? What is the extrapolation?

In the same domain of weapons and war, the answer is clear. The extraordinary change in potential scale of warfare which seized the world in its grip between 1932 and 1952 has ceased. In 1962 the most striking new weapon was perhaps a multiple-warhead Polaris, which could drop its 100-kiloton load of three nuclear weapons in a shotgun pattern at a range of a thousand miles or two. In 1972 the tested device was the accurate Lance guided missile, with a warhead still below the megaton scale, and a range of 100 to 200 miles, accurate and light enough to be mobile by road. It takes no close study to conclude that these weapons, powerful and numerous though they are, and important for military plans for fighting or deterring nuclear war, in no way take the world to a new domain, as did the decades 1932 to 1952.

I think that is the message of the anniversaries. We do not know any surprise latent in the universe explored by science which offers the dominant effects of nuclear energy, and nuclear energy has been pushed to its physical limits. There is microbiology, biochemistry, neurobiology, the new meteorology; each of these has within it some omen of a possible extraordinary weapons. Yet none has in fact appeared, and I am optimistic enough to expect that indeed no such novelty as the nuclear one lies behind the veil of the future. The weapons of the past 20 years have been exploitations of control, of propulsion, of miniaturization, of accuracy, and the like. They contain power, both economic and military, possibly even decisive power. But in the nature of things they are signs of continuity, not of the revolutions implied by neutrons, chains, and the thermonulear reactions of the stars brought to human scale in the years we celebrate. The new terrors arise not from the laboratory but from the budget. It is multiplicity we have to fear, not novelty.

Indeed, if I am asked to describe the most important change of the year 1982 in the expected nature of warfare, it would not be the Tomahawk long-range cruise missile tests, or the doubtful MX missile, or even the new Trident submarine. They are to be expected, and enter the already-threatening catalogue of too common things. What is new, what offers hope, what may make a change before which all else will fade, is the million serious people gathered in mid-Manhattan in early June 1982. There is hope for a warmer anniversary in a decade or so to come. □

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Harold M. Agnew

Early impressions

After being associated for 40 years with our nation's nuclear program it's difficult to select a particular incident which stands out above others. It may bemuse my colleagues to know what events I do not choose to write about on this occasion: events such as being with Enrico Fermi on December 2, 1942 under Stagg Field; or being over Hiroshima on August 6, 1945; or playing touch football with Bernie Feld and the *real* Julius Ashkin (as opposed to the imposter who used Ashkin's name) on the Chicago Midway; or making a one-gram radium beryllium source on an afternoon in New York, putting it in a briefcase, and flying back to Chicago with it under my seat.

For me one of the most interesting incidents happened before any of these. Appropriately, it is related to safety in nuclear activities, an issue which is still of paramount importance. Perhaps it stands out in my memory because of all the misinformation promulgated by the Fondas, Naders and Jerry Browns of the world with respect to public safety and nuclear systems, and their implications that those involved with nuclear energy are not vitally concerned with all aspects of public safety.

The first endeavors to understand the factors involved in creating a self-sustaining man-made nuclear chain reaction were carried out by Fermi and his colleagues—Herb Anderson and Wally Zinn, in particular—on the campus of Columbia University. Fermi had erected a graphite pile, known as an exponential pile, approximately eight feet on a side, but the results of the experiment were not as favorable as he had anticipated. (Al Wattenberg keeps telling me the pile wasn't that large but I remember it as being very large.) Because of poor quality control, the materials being employed—graphite and uranium oxide—were quite impure and the magnitude of the impurity was not then fully appreciated by the Columbia team. The team decided that the cross sections being used were probably in error, that the oxygen and nitrogen in the air might be absorbing neutrons more than anticipated, or that carbon was not as good a moderator as expected. In any event Fermi decided to encapsulate the whole graphite pile in a sheet metal structure and to evacuate the air from the pile. However, having removed the air by using an array of mechanical vacuum pumps, the results were still not as favorable as he had anticipated. Fermi then decided to supplement the graphite moderator with a hydrogenous material and a petroleum gas, butane, was chosen. One must keep in mind that all this was taking place on the Columbia University campus in the middle of New York City.

Work had begun in Chicago at the time, and a group from Chicago under John Manley had been sent to Columbia to attempt to wrap up the experiments which Fermi had started. The concept of the chain reaction was easy to grasp, but its realization was to be an event with which man had had no experience—and therefore no idea of the time constant of the multiplying reaction. Because of true ignorance the nuclear safety aspects were not initially predominant. The real issue was whether the concept could be achieved. It's hard to worry about something in detail when you simply don't know what to worry about. In any event I, at age 21, had not been made aware of any potential nuclear safety issues.

After the decision to fill the evacuated pile with butane, plans were prepared to introduce the gas into the eight-foot cube. But at that point Elizabeth Graves, known as "Diz," raised the question of how we were going to remove the gas after the experiment was concluded. For reasons which I don't recall, cryogenic pumping, which would have been quite safe, was not considered, and all other schemes clearly presented safety problems which in the end convinced Fermi that the risks involved were too formidable to take. As a consequence, work on that pile ceased and the total effort was transferred to Chicago where a larger pile, using purer materials, was employed. History was made on December 2, 1942 and confirmation of the ability to create a nuclear chain reaction occurred in Chicago, and not in New York City at Columbia University.

With all the concerns evinced about nuclear safety today I am continually reminded that even in the beginning safety was a paramount issue. But in

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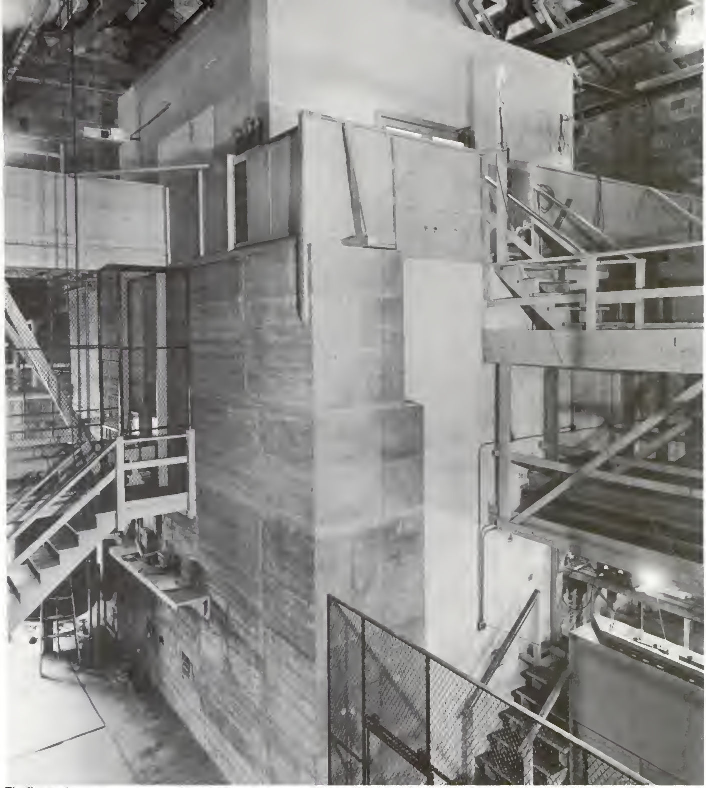

The first nuclear reactor, constructed in a squash court beneath the West Stands of Stagg Field at the University of Chicago.

those times the first safety consideration of which I was made aware was one relating to a chemical explosion. If, in Chicago, the phenomenon of delayed fission neutrons had not proved to be a reality—and had only prompt neutrons been available, the history of nuclear energy might well have been quite different. The incident at Three Mile Island has shown that emphasis must be placed on enhancing the safety of the utilities investment and plants must be designed which are much more forgiving to human and mechanical failures. Such technology does exist and we should pursue it with vigor.

It is true that we seem to worry more about those matters with which we are familiar than those about which we know little—and this is probably fortunate. Otherwise, there might be no technical or social programs and life would cease to present the constant challenges and excitement that accompany original experimentation and exploration on any scale of endeavor. All of this offers the potential that tomorrow will be better than today. ☐

With the discovery of the neutron, the way was opened for a possible release of the energy locked up in the atomic nucleus. Otto Hahn and Fritz Strassman discovered nuclear fission in 1938, and the stage was set for the next great step—the man-made nuclear chain reaction.

ALBERT WATTENBERG

December 2, 1942: the event and the people

On Ellis Avenue between 56th and 57th Streets at the University of Chicago is a bronze plaque with the following inscription:

"ON DECEMBER 2, 1942, MAN ACHIEVED HERE THE FIRST SELF-SUSTAINING CHAIN REACTION AND THEREBY INITIATED THE CONTROLLED RELEASE OF NUCLEAR ENERGY."

If the event had been only the birth of a new era allowing the peaceful and safe use of nuclear energy or the development of a new and powerful research tool, it would have been wonderful. Unfortunately, it also made possible the production of sufficient plutonium to make nuclear fission bombs. This latter possibility and the threat of war had provided the motivation and support needed for the rapid development of the first nuclear reactor or first chain-reacting pile, CP-1.

Forty-two people were present at that event. I was one of about two dozen physicists in their twenties who had been helping to build the pile or the instrumentation for it. Enrico Fermi had asked me to join him at Columbia University 11 months earlier. Most of the others were also there because physics professors at their schools had asked them to help.

While a college student in the 1930s, I had helped organize meetings and demonstrations against both war and fascism. With Hitler's subjugation of most of Europe in 1940 and 1941, the fear of fascism in the United States became real and very personal. The announcement of the bombing of Pearl Harbor interrupted a concert to which I was listening; it was tremendous shock. In the days that followed, there was a great deal of patriotic fervor. However, my real fear was of the Nazis and not of the Japanese. Having decided to enlist as an ensign in the Navy, I asked for an application at the Columbia physics department, where they were available. But the head of the department insisted that I talk to Harold Urey, John Dunning, Lucy J. Hayner, and Enrico Fermi. They all knew me and asked me to join the projects they were running. Fermi had known me mainly as a student in the first quantum mechanics course he taught at Columbia in 1939, and we had talked several times outside of class. He certainly was my best teacher in graduate school. He had a very pleasant and easy-going style. He made everything look very logical and straightforward, and he never made any mistakes. (It took eight years before I found him making a mathematical mistake.) If I had an awe of Nobel-caliber physicists, I was not conscious of it since I had also taken courses from Urey and Rabi, who were not great teachers.

Exponential piles at Columbia University. I was very pleased that Fermi wanted me to work with him. When I joined the group in early January 1942, it consisted of Herb Anderson, Bernie Feld, Enrico Fermi, Leo Szilard, George Weil, and Walter Zinn. In February or March, Harold Agnew and John Marshall were sent from Chicago to help us complete a program of measurements before the group moved to Chicago. We all, except Leo Szilard, took part in making the measurements, in calculating the results, and in constructing the assemblies of graphite and uranium oxide.

Szilard quite correctly felt that his talents were better applied to things other than routine physics work. For example, he played a major role in persuading others to get financial support for the work from the U.S. government. It was Szilard who persuaded Einstein to write to President Roosevelt to alert the United States to the possible military applications of nuclear fission. Szilard also understood very early the importance of having very pure graphite and uranium, and he devoted a good deal of effort to procuring the test materials. Fermi, devoted to the theory and practice of physics, was very pleased to leave to Szilard the task of persuading industrial companies and other people to provide the needed materials. Szilard was an idea man, and it seemed to me that he and Fermi interacted rather well. Together, they were responsible for the very important idea of placing the uranium oxide in a lattice in the graphite instead of spreading it out uniformly.

Szilard was not discouraged by the fact that uranium metal had only been made in very small quantities. He was very persuasive in influencing laboratories and industries to produce larger amounts. High-purity graphite and some uranium metal did arrive in time to complete the project on schedule.

By contrast, Fermi acted as an equal in the laboratory. He did his share of building and measuring with the rest of us during the day. While we were making measurements, we worked day and night, and the younger men who were not married took the night shifts.

I recall that a good deal of my training in the use of Geiger counters was

Tenth anniversary reunion honoring the first self-sustaining nuclear chain reaction. The participants gathered once more under the West Stands.

by Bernie Feld; he and I both had an inclination to work at night. After a while, I was on the night shift alone, and the measuring routine was exceedingly rapid, with no rest. We measured the radioactivity in indium foils every five or ten minutes with Geiger counters. While accumulating the data, we carried through the calculations. The following morning, Fermi would check on all of the numbers that we had obtained to make sure that we had not made either numerical or clerical mistakes. He always made a personal summary of the measurements.

The indium foils had been made radioactive by being put in a neutron flux; so we were really measuring the neutron intensity. Fermi had a ritual: we made two measurements; then we ran a standard to check that the counters were reliable; then we measured without a foil or a radioactive standard in the Geiger counter. Fermi was always running checks on everything he did, especially on the reproducibility and reliability of the equipment with which he was working. This was a terrific training—when we encountered a new effect, we could be pretty sure that it was not due to our instrumentation.

The neutron intensity (flux) measurements were to ascertain whether a given lattice of uranium oxide lumps imbedded in graphite could give a self-sustaining chain reaction if the dimensions of the structure were sufficiently large. The results of the experiment could be expressed as a determination of a multiplication constant k, or reproduction coefficient. It is the fundamental property of the lattice structure and the neutron interaction probability of the material. It can be thought of as the average number of neutrons produced in the first generation by a primary neutron in a lattice of this structure if it had infinite extension. The Columbia group had developed this "exponential pile" technique the year before.

First they had studied a cubic lattice with the uranium oxide in the form of loose powder. When I joned them, they were starting to press the uranium oxide, and they had changed the lattice size. With a neutron source near the bottom, the neutron distribution decreased exponentially toward the top. The exponent, or the rate at which the exponential decay occurred, depended on the size of the structure, a measure of the properties of the graphite, and the multiplication constant k. The size dependence arises because of the neutrons lost by leaking out the sides of the structure. The exponential piles were not small—about eight feet by eight feet on the base and about 10 feet high. If you made a pile with just graphite and no uranium oxide, you could determine the properties of the graphite. From doing such measurements, we came to the conclusion that the graphite had serious amounts of impurity in it. By March 1942, we knew we needed purer graphite in order to obtain a self-sustaining reaction even in a pile of infinite size.

One of the last measurements we were trying to complete at Columbia was to determine the effect of gases other than air being in the interstices of the graphite. We were concerned that nitrogen acted as an impurity and that there was an appreciable amount of it in the porous graphite. We therefore wanted to put this large structure inside a sheet metal can, which required soldering together many strips of sheet metal. We were very fortunate in getting a sheet metal worker who made excellent solder joints. It was, however, quite a challenge to deal with him, since he could neither read nor speak English. We communicated with pictures, and somehow he did the job. We put a vacuum pump on this canned exponential pile, pulled the air out of it, and then filled it with carbon dioxide. From this it was established that one could increase the multiplication constant if one got rid of the nitrogen in the graphite. This result led to Anderson's obtaining a big cubical balloon to enclose the reactor that finally became self-sustaining.

Water in the graphite or uranium was an undesirable impurity. I have recollections of Bernie Feld building an oven that must have been 20 or 30 feet long to try to drive the water out of either the uranium oxide or the graphite bricks. I also remember spending the nights there to make sure that fires did not get started from all of the electrical heating rods.

After measurements of the pile in the tin can were completed, we put the graphite into numerous cardboard cartons. We glued the boxes closed, then shipped them, the Geiger counters, and the rest of our equipment to Chicago. At the outbreak of the war, Arthur Compton had been put in charge of the project, and after a few indecisive meetings, he decided that the project for "chain reacting piles" (subsequently and more appropriately called nuclear reactors) should be moved to the Chicago area.

Albert Wattenberg is professor of physics at the University of Illinois, Urbana-Champaign (61801), and a fellow of the American Physical Society. In addition to collaborating on the first chain reaction, Wattenberg helped design, assemble and test the first enriched uranium heavy water reactor in 1949.

Fermi, Zinn, and Anderson. In April 1942, we left New York, and Bernie Feld, Enrico Fermi, and I started living at the International House of the University of Chicago. Enrico Fermi was there because Laura and the children were still in New York. Fermi and I frequently ate dinner and spent the evening together. He told me that his work habits were better than mine—he stopped working at about 5:00, relaxed after dinner, and went to bed early. He could beat me at chess, but I beat him at ping pong. There was a continuous game of a student-teacher relationship when some of us were with him. If we saw something, he would ask us to explain quantitatively what was happening. For example, the fire in a fireplace led to his making us try to calculate the amount of vacuum above the fire; seeing a dirty window, he asked us how thick can the dirt on a windowpane get? To play the game you only had to know the fundamental constants of nature and have some idea as to how things might vary with one another. If you got off to a poor start, he would help you by asking you to go to some extreme condition where the answer was obviously ridiculous. He was very quick in thinking up such tests which he used when he made his own formulations. He gave us the wonderful feeling that we also could and should know all physics.

Fermi enjoyed teaching and wanted those working with him to understand what was going on. This led him to give two sets of courses in neutron physics at Chicago, which prepared many of us for the events that took place on December 2. What made him a wonderful teacher was that he would avoid proofs that were too esoteric or lengthy. Instead, he would develop a plausibility argument so that we felt it was almost obvious.

When we went to Chicago, the original New York group was split into two main groups—one under Zinn and another under Anderson—and an appreciable number of additional people were brought in to work with us. Another group under Volney Wilson was in charge of the controls and instrumentation development. Before the war, Wilson understood what nuclear bombs would do, and he had been unwilling to work on the project. However, in the fervor created by the war, he decided he should work on the project.

As well as measuring exponential piles, Zinn took charge of the machining of the graphite and the pressing of the uranium oxide. I feel that Wally Zinn's enormous contribution and cleverness in this project have not been adequately appreciated. Although Fermi understood the physics, could teach it to the rest of us, and had developed the techniques for making measurements, Zinn really had a full grasp of the steps and processes needed to build a pile. He was exceptional at devising straightforward, reliable, and efficient solutions to an enormous variety of physics and engineering problems. He arranged for the personnel and equipment needed to get the job done. With about half a dozen young physicists, one millwright, and about 30 kids who had dropped out of high school, he carried a major share of the physics measurements of the exponential pile program, and he machined all the graphite blocks and pressed the uranium oxide briquettes for those exponential piles and for the first self-sustaining pile. Zinn drove people hard; he obtained very high quality work from them and also gave them a great deal of satisfaction in accomplishment.

Zinn had no draftsmen until 1943 when he took charge of designing the first heavy water reactor, although I did some drafting for him a couple of times. The loose uranium oxide powder needed to be pressed to a density as high as possible. He designed a die that was built at Columbia. At Chicago he designed beautiful new dies that would make the lumps somewhat spherical in shape (we called them pseudo-spheres). He probably was assisted in decisions on the dies by a very bright and superior tool and die maker in the Physics Department shop, whose name, I believe, was Dicostanza. The oddly shaped die and all its parts had a fantastic polish, which meant that we could minimize the use of lubricants and avoid getting the lubricant on the uranium oxide lump. The lubricant would have absorbed neutrons. Zinn also had the foresight to have some spare dies made.

Prior to 1939, Zinn had been a professor at the City College in New York. He had had his own independent research program studying neutron interactions at Columbia University. After fission was discovered, he and Szilard began doing experiments together; sometime in 1939, they joined forces with Fermi and Anderson. A very interesting account of this period and the entire war period is given by Herb Anderson in the publication *All In Our Time.*[1]

Herb Anderson was already a very advanced and experienced graduate student in 1939 when Fermi arrived. He started working with Fermi almost immediately. Anderson had experience with experiments at the cyclotron, and this permitted them to study the very important question of the production of neutrons in the fission process. I gather that Anderson became a family friend; since he knew how things were done in America, he was helpful to the Fermis in their initial period in the United States. He also was very effective in arranging to get equipment and the other things needed to carry through the program. He was exceedingly conscientious about keeping up with the theoretical aspects of the physics with which he was involved.

Fermi involved Anderson in a very

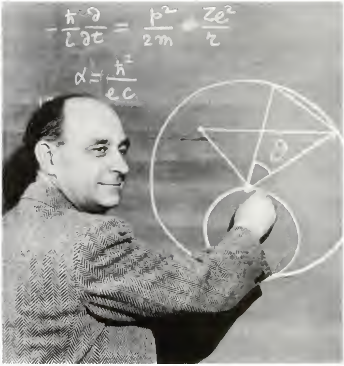
In April 1942, we left New York and Bernie Feld, Enrico Fermi and I started living at the International House of the University of Chicago.

broad variety of experiments. As well as studying the fission process itself, they put a great deal of effort into establishing standards for neutron measurements. He studied the spectrum of neutrons from various sources. The neutron sources used in the exponential piles consisted of radium mixed with beryllium powder; Anderson was the expert on preparing these sources. Probably due to working with the beryllium powder, after the war he developed berylliosis, an illness which affected much of his later life. Bernie Feld and I were both trained by Herb in how to prepare these radium-beryllium sources. Anderson and Feld went to Los Alamos in 1944; since they could not travel, I ended up making neutron sources for the entire Manhattan Project.

The sources consisted, in many cases, of as much as a curie of radium. At 10 centimeters from a curie of radium in equilibrium with its decay products, a person would be exposed to the order of 100 roentgen per hour. The soldering in such work required great quickness and sureness. Actually, in making the radium-beryllium sources, we worked with radium which had had all of the radon driven out of it; a radium solution containing beryllium powder was boiled down to a dry powder which we transferred into brass containers which we sealed by soldering. We had several hours to work before the radioactivity built up to a very dangerous level. After the radium was sealed up for several days, however, it came into equilibrium with its radon decay products and then presented the hazard of a full curie of radioactivity. I repaired several sources of this type in which the solder joints had broken. Subsequently my white blood count dropped to a level of about 4,000, about half my normal count. From my work with the radium sources during the war and making up these neutron sources at other national laboratories, my blood count remained low for a number of years.

Exponential piles in Chicago. The exponential piles that we had built at Columbia had given results indicating that even an infinite amount of material would not lead to a self-sustaining structure. This was mainly because of the impurities in the graphite. During the spring, some new graphite arrived, and the exponential piles we built and measured in Chicago indicated that it would be feasible, with a very great deal of material, to build a chain-reacting structure; thus, it was very worthwhile to continue to get the better quality graphite and also to improve the quality of the uranium oxide that we were obtaining. As well as Szilard, Ed Creutz and Frank Spedding were also involved in this effort. Those of us doing the exponential pile work were not involved in discussions of the procurement of better materials, and I think that we didn't really appreciate the efforts that were going into this, both by people in the project and by those in the industrial companies. I do not remember these procurement problems being discussed at the general meetings that were held in the spring and summer.

My recollection is that general meetings took place in the evenings either every other week or once a month, and many of us attended. They were interesting because other groups discussed their experiments, calculations, or problems. The impressive array of people in the theo-



Enrico Fermi, leader of the scientific team that achieved the first controlled, self-sustaining nuclear chain reaction.

Fermi (far right) being admitted to the Italian Royal Academy, 1929.

Zinn drove people hard; he obtained very high quality work from them and gave them a great deal of satisfaction in accomplishment.

retical group under Eugene Wigner included Al Weinberg, Bob Christie, John Wheeler, Gale Young, and others; and a group using the Chicago Cyclotron under Snell was studying individual aspects of the fission process, such as the losses of fast neutrons.

The motivation for the project, to produce plutonium for a bomb, led to a very large effort on the chemistry and the separation of plutonium from uranium. Glenn Seaborg was the head of the nuclear chemistry division; James Franck joined that division in the fall. Quite a few engineers were studying the designs for various alternative large-scale chain reacting assemblies. A few of the engineers were good; but some of the engineering firms were very stupid. One engineering firm we got rid of had hired new people to work with us, and they seemed to have specially selected second-rate people for us and to have kept their first-rate people in their home office working on other projects. In the winter the DuPont Company joined the project, and they sent some of their best people. It was a real pleasure to train them.

Although we were committed to building a graphite-uranium pile, other possible systems were always being considered and some measurements of them had to be made. During the summer, according to my recollection, we tried to measure the multiplication constant of a beryllium-uranium oxide system. Beryllium metal certainly has very excellent characteristics as a neutron moderator in a reactor. However, it was not available in large quantities, and it would have been very difficult to machine. At the time, we did not know about the physiological hazard of handling beryllium; many people would have been hurt if we had tried to build a pile of beryllium metal.

From the middle of October until December 2, we were on a regime of about 90 hours of work a week. All we did was to work and sleep, and sometimes we didn't even get to eat meals. Sometimes we thought of why we were doing it; several times we discussed what we would do if the Nazis won—where we would try to hide in the United States. We were fairly certain we would be killed if we were caught. One morning very early Dr. Alvin Graves came in (he was slightly older than the rest of us) and wanted to take over what I was doing, although he wasn't due in until late that afternoon. He said he just couldn't sleep. He felt the Nazis were working, that they were pushing ahead to get there before us. We were in a real race, and he felt he shouldn't be taking a day off. But when you are working 90 hours a week, you listen to the news on the radio while dressing, only glance at newspapers, and spend little time discussing the world situation—you keep functioning and solving the problems that arise in your work.

In addition to running the factory, we shared the shifts in measuring the neutron fluxes in the exponential piles. Early in October we had machined enough of the new high density purer graphite, and we had pressed enough new uranium oxide pseudospheres to build exponential pile 18. This was to test whether the pile would be self-sustaining with the amount of new material that would arrive by December. When we had completed measurements, we knew that such a graphite-uranium pile would work. From Fermi's lectures, we understood the significance of what we had measured and how big the pile would need to be in order to be chain-reacting. Two additional improvements would make it a certainty. The big balloon-cloth bag purchased by Herb Anderson would allow us to get rid of the air in the graphite and therefore reduce the amount of neutrons absorbed by the nitrogen in the air. The other improvement was that some uranium metal, which is much better than uranium oxide, would be arriving and could substitute for the oxide, especially in the center of the reactor.

Until the end of September, the preparation of the materials and the construction of the exponential piles were carried on mostly by Zinn's group. Anderson's group had the responsibility for the standardization of neutron measurements. In October, in order to get all of the work done, the groups under Anderson and Zinn were combined, and other physicists joined us on a temporary basis.

The graphite and uranium oxide factory. The first self-sustaining pile was designated CP-1, Chicago Pile 1. For it to be self-sustaining, we needed to press about 22,000 pseudo-spheres of uranium oxide, and we had to machine about 400 tons of graphite. The graphite was received from the manufacturers in bars that were about four and a quarter by four and a quarter inches in cross-section and in lengths that varied, depending on the manufacturer, from 17 inches to 50 inches. Surfaces were quite rough, and therefore it was necessary to make them smooth and to cut the bricks to an accurate standard length. For the lengths we used a woodworking cutoff saw and it turned out that woodworking machines were the best thing to use. For the cross-section, two of the surfaces were made plane and accurately perpendicular in a jointer, and then the other two surfaces were brought to the size we wanted—four and an eighth by four and an eighth—by running them through the planer. These machines were set up and maintained very well by Gus Knuth, a millwright and carpenter with a great deal of ingenuity. The rest of us who worked with him most of the time were physicists and a motley

From the middle of October until December 2, we were on a regime of about 90 hours of work a week.

crew that had been rejected by war industries. The graphite machining produced black graphite dust all over the place. We breathed it, slipped on it, and it oozed out of our pores, even after we washed and showered. Everyone dressed for this work in coveralls, and a young professor could not be distinguished from the kids we hired from the area of Chicago known as Back of the Yards (the livestock yards).

One quarter of the graphite bricks needed to be accurately drilled to provide three and a quarter inch diameter holes shaped to fit the uranium oxide pseudo-spheres. Each hole was drilled in a single operation by using the lathe in an unorthodox fashion: we put the graphite bricks where the tools should be and a special homemade spade into the rotating chuck of the lathe. This rickety lathe wasn't even second-hand—it was probably fifth-hand. These tools required frequent sharpening, which proved to be time-consuming. We had tried carballoy bits but rejected them. Instead, we made the drilling tools from old files. Harold Lichtenberger, Bob Nobles and I took turns at shaping and re-sharpening these tools. Between 60 and 100 holes per hour could be drilled. After drilling 50 to 70 holes, however, a bit had to be resharpened, so about 30 bits a day had to be reground. We did not have a jig, so we did it by eye.

We handled the 400 tons of graphite a number of times, unloading trucks, storing it, machining it. Then we either used it in an exponential pile or stored it again until we put it into the pile. One day we received a telephone call saying that a shipment of graphite was in Chicago at a railroad yard, but that they could not get it unloaded. This provided an interesting sociological insight. The truckers who unload freight cars use itinerant labor, mainly picked up at the flop houses on West Madison Street in Chicago. Apparently during the war there were more jobs than men, or they had enough money to sleep several days and nights, so the truckers couldn't get any itinerant labor to work for them. When we learned this, we of course went down and unloaded the freight car ourselves. A carload contained around 50 tons of graphite—in this case about 2,000 bricks weighing about 50 pounds each. Four of us unloaded the freight car in less than a day and then went back to work.

The uranium in the pile was mostly in the form of uranium dioxide lumps, the remainder in the form of about six tons of uranium metal which we simply had to put into the holes that we had already drilled. We originally planned to press 22,000 uranium dioxide lumps from loose powder. We had an old press and the dies designed by Wally Zinn. On a good day, working three shifts and using two dies, we could press about 1,200 lumps in 24 hours. At the beginning, a team consisted of three Back-of-the-Yards high school kids and usually one young physicist like Lichtenberger, Nobles, Warren Nyer, myself, or others. At the end of October, the Back-of-the-Yards kids were drafted, and more physicists were used to help us. The work in the press room was fast and monotonous. To keep up a fast pace, we frequently sang. We made mistakes only a couple of times during the whole period.

When we had completed about three quarters of the pressings needed, one of the poles of the press cracked. The press was another example of the third- or fourth-hand equipment that we were using. We reduced the pressure a little bit so that we didn't break the press completely. We sang a little louder and kept our fingers crossed.

After we had been doing this for quite a stretch, some physicians walked in with cages full of mice. They were putting them in the room because of the uncertain toxicological effect of breathing and eating uranium oxide. The medical experiment was to see how long the mice would live breathing uranium oxide dust. The mice would be there 24 hours a day; we would be there only 12. The physicians also brought dust masks for us to wear. It is very difficult to get people to start wearing dust masks, especially if they are smokers who like to sing while they work! The Back-of-the-Yards kids refused to wear the masks after a short while; and the physicists did not set a very good example.

The University Commons was the most convenient place to eat, but it was an appreciable walk, especially on a cold and snowy day. There was only one—very miserable—lunch counter close to where we worked. The only edible thing the little old lady could make was mashed potatoes with gravy; even her hamburgers came out like shoe-leather. In that period, for many of us, smoking was a way to avoid getting hungry so that we could skip meals. However, the cigarettes available during the war were terrible. A pack of Fatimas today would be a collector's item.

Preparing for December 2. Harold Lichtenberger came from Decatur, Illinois and had just received his B.S. in physics. He was observant and quick to grasp new concepts in physics. His previous work experiences were very different from the rest of ours: he had worked in the repair shop of a locomotive yard and knew how to handle large, heavy equipment. The rest of us learned a great deal from him.

The floor level of the squash court in which we were going to build the pile was below the level where we were doing all of the machining of the graphite. We had to be able to bring skid loads of graphite bricks down to that floor. A portable elevator was obtained (I assume by Zinn), and was

The big balloon-cloth bag purchased by Herb Anderson would get rid of the air in the graphite and reduce the amount of neutrons absorbed by the nitrogen in the air.

delivered to the door of the West Stands of Stagg Field. Harold Lichtenberger played a major role in getting that elevator moved the hundred or so feet from the entrance to the squash court. Then we had to get it down onto the squash court floor and arrange to get it right side up. We enjoyed new challenges like rigging.

Prior to the arrival of the elevator, we had passed the graphite blocks by hand from the floor up to physicists who were on a wooden scaffolding. We would work 8 to 12 feet off the floor on 2 by 12 inch planks. General Leslie Groves walked in on us while we were building an exponential pile, and he was upset. I suspect that we got the elevator to avoid his criticism. After a while, most of us were very much at ease walking out on the planks, although a few did not adjust to it. About sixteen of us helped build the exponential piles, then irradiated and measured the indium foils.[2] While we were at the West Stands, Anderson's group was responsible for the Geiger counters and the associated electronics.

Fermi's monthly report for October indicates that we built and measured seven exponential piles, as well as running the factory. Several of the exponential piles were to study the reproduction factor that could be obtained from using uranium metal produced by Westinghouse. We tried to find the optimum amount of metal to use in the graphite lattice. The results were very encouraging and showed an improvement in the reproduction factor of more than 3 percent over that obtained with uranium oxide. The arrival of additional uranium metal in time to be placed near the center of the reactor certainly reduced the size of the pile from what we had originally thought would be necessary; it also saved us from having to use the balloon bag to remove the nitrogen from the graphite.

Several of the other exponential pile experiments were not directly related to the building of CP-1, and they show that a large effort was already underway in planning the pilot and production reactors, on the assumption that CP-1 would succeed. In one exponential pile, we simulated the situation which would exist in a water-cooled reactor. The measurements were to provide data for Eugene Wigner, Gale Young, and the theoretical group who were studying systems for water cooling the production reactors. Another exponential pile was for studying the possibility of using liquid bismuth as a cooling agent, one of the possibilities on which Szilard wanted data. Years before, Szilard and Einstein had developed an electromagnetic pump for liquid metals. After the war, such pumps were used in prototype liquid metal cooling systems.

We had planned to move the material to a new laboratory at the Argonne Forest near Chicago. But when the construction of the building was halted due to labor problems, Fermi decided to proceed to build CP-1 in the squash court of the West Stands. On November 16, the balloon-cloth envelope was erected in the squash court and a circle was drawn to indicate where the first graphite bricks should be placed. My recollection is that Al Graves placed the first bricks. A reactor in the form of a sphere would lose fewer neutrons than one in the form of a cube. To make it spherical, a wooden understructure was built in which to place the graphite. In addition to everything else he was doing, Herb Anderson went around to the lumberyards to get the wood. Gus Knuth, the carpenter, cut the wood and built it into the shape that Fermi calculated would support the bottom of the pile.

I believe that we must have put on two layers per shift, one with the uranium oxide in the bricks and one without it. The day shift consisted of those of us in Zinn's group, and the night shift consisted of Herb Anderson's group, with some additional people. During the construction, Fermi had a meeting in his office every morning with Zinn and Anderson to plan each layer. We had different types of material, and for each type we had measured the reproductive properties in the exponential pile experiments.

The West Stands of Stagg Field at the University of Chicago, site of the first reactor.

The machining produced black graphite dust all over the place. We breathed it, slipped on it and it oozed out our pores.

There was a very great advantage in putting the material with the best reproductive factor in the center of the pile. I provided Zinn and Fermi with the inventory of all this material. For each layer, Fermi specified which types of material should be toward the outside of the layers and which types should be toward the inside. He used uranium metal instead of uranium oxide in the central area of the pile, making optimal use of the material we had.

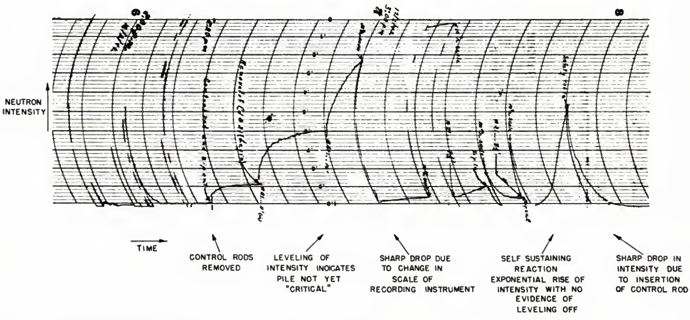
About eight weeks prior to this, Fermi had started a series of weekly lectures which continued until November 20. Two of the lectures described the measurements which can ascertain when a pile will be critical. Another covered the time-dependence to be observed. He showed that, approaching the critical point, the exponential rate of rise of neutron intensity becomes slower and slower. When the pile is subcritical, but approaching the critical point, the neutron intensity levels off at increasingly larger values; the intensity approaches infinity nearing the critical point. The approach to the critical point could be effected by adding layers to the pile, or, after the pile is constructed, by pulling out a cadmium rod which has prevented the pile from reacting. Those of us involved in running the factory and exponential measurements would stop for Fermi's evening lectures, which were a beautiful, simplified treatment of reactor theory. Even someone unsophisticated in mathematics could still understand what was known, what was happening, and what to expect.

The actual steps he followed on December 2 in taking the pile up to and then above criticality were based on the formulas given in these lectures.[3] They clearly demonstrate that there was good understanding of pile kinetics, despite implications to the contrary in some melodramatic press accounts of the events that took place on December 2, 1942. A few of those press accounts claimed that we didn't know whether we were going to blow up Chicago, and other such misleading information.

The measurements Anderson made every night used two types of neutron detectors, one a BF_3 counter built by Leona Woods, and the other the method of irradiating an indium foil overnight as closely as possible to the effective center of the partially built pile. Its activity was then measured the following morning on a Geiger counter, and results from the foil measurement were compared with the BF_3 readings. The neutrons multiply-

DECEMBER 2, 1942, Start-up of First Self-sustaining Chain Reaction

NEUTRON INTENSITY IN THE PILE AS RECORDED BY A GALVANOMETER



It was 11:30 a.m. and Fermi said 'I'm hungry. Let's go to lunch.' The other rods were put into the pile and locked.

ing in the structure were those that arose from the spontaneous fission of uranium-238. We now had sufficient uranium in the pile so that the natural neutrons were adequate in number, and we no longer needed to use a radium-beryllium source.

As a matter of precaution during construction, long before we were near the critical layer, some cadmium strips were inserted into some of the slots and were kept locked there. They were removed once every day with proper precautions to check the approach to the critical condition. I believe that we reached 52 layers November 30, and Fermi's extrapolations showed that the pile would be just critical when the fifty-sixth layer was added. Fermi decided to add the fifty-seventh layer so that it would be just one layer above critical. Anderson and his group added the fifty-seventh layer the night of December 1 with the agreement that they would not pull out the cadmium rods to see if we had been successful. He adhered to the agreement, even though it was very tempting not to.

We were all informed about the results of these measurements, so we knew on December 1 that Fermi would be withdrawing the control rods and making critical measurements on December 2. The construction of the reactor had been completed about a week earlier than Compton, the director of the project, had anticipated. The pile was somewhat smaller than the October estimates, partly because more uranium metal had arrived during November; therefore it was not a spherical structure but somewhat flattened at the top. Also we did not need to remove the nitrogen from the graphite, so the balloon did not need to be closed up.

The group under Volney Wilson, who had been working on instrumentation and controls for monitoring and controlling the first pile, was also alerted to the plans for December 2.[4] They had built redundant boron triflouride counters and ion chambers; the former are good for lower neutron intensities, the latter for higher neutron intensities. Most of the equipment used 110-volt power supplies; a few operated off batteries. Overbeck had built some automatic control rods that could be operated remotely with small motors. On December 2, these were kept out of the reactor.

We also had some cadmium rods which could be slid in and out of the pile by hand; one of these was used on December 2. The pile was at one end of the large squash court, and a balcony was at the other end. The slots in which the control rods and the safety rods slid were extended outside the pile on a framework that reached almost to the balcony or the wall below it. We had several different types of safety rods. One of them, called ZIP, which was designed by Zinn, operated by gravity. A solenoid held the rod out of the reactor, while the other end of the rod was tied to a rope which went over a pulley and was attached to a weight. The solenoid was attached to a relay operated from one of the detecting devices so that if the electricity failed or if the intensity became sufficiently high, the solenoid would release ZIP. Wilson's group had built a series of safety rods which could be activated remotely by pushing a button. An additional gravitational safety rod similar to ZIP was tied out with just a rope on the railing of the balcony where we stood. Norman Hilberry had an axe to cut that rope if human intervention were required to shut the pile down in a hurry.

December 2, 1942. On December 2, I arrived at about nine o'clock. I checked out some supplementary electronics and detection equipment that Wally Zinn and I had set up in a little tunnel on the west side of the squash court containing the pile.

Sam Allison had arranged for three large jugs of cadmium sulfate solution to be brought over and put on the elevator near the top of the pile. He thought that the jugs could be carried onto the pile and poured onto it in case of unforeseen disaster. Several of us were very upset with this since an accidental breakage of the jugs near the pile could have destroyed the usefulness of the material in the reactor. In fact, one cadmium rod pushed into the center would have been equally effective. Because he was such a nice guy, however, and a big shot, we did as he asked. I do not remember which three people were on the elevator in the morning with the jugs of cadmium sulfate. The rest of us gathered on the narrow balcony at the other end of the squash court. At one end of the balcony were the electronics, scalers, and a pen recorder attached to detectors on the pile. Some of the younger physicists would read and plot the readings from the scalers; Fermi could look at their data or at the pen recorder; they were independent measurements of the neutron intensity.

On December 2, we began by checking that the neutron intensity was the same as Herb Anderson had measured the previous night, when all except one of the cadmium rods in that pile had been removed. The rates on some of the other instruments were checked and some adjustments were made in anticipation of the neutron intensity's increasing as we proceeded in the morning.

Fermi planned to use the last cadmium rod in the pile as a control rod. It would be set by hand at various positions so that we could measure neutron intensity for those positions. He had calculated in advance the intensity that he expected the pile to reach when it saturated at each of these various positions. George Weil was in the squash court in a position to be able to move the last rod. After

The pile was functioning exactly as expected. Fermi broke into a big, cheerful smile. He put away his slide rule and announced, 'The reaction is self-sustaining.'

the checks on the instrumentation were completed, Fermi instructed Weil to move the cadmium rod to a position which was about half-way out. It was well below the critical condition. The intensity rose, the scalers increased their rates of clicking for a short while, and then the rate became steady, as it was supposed to.

While it was rising, Fermi periodically read some numbers and did a quick calculation on his little slide rule of the exponential rate of rise of the neutron intensity in the pile. After the intensity had leveled off, he then told Weil to move the cadmium rod another six inches. The neutron intensity in the pile rose further and then again leveled off. The pile was still subcritical. Fermi had been busy noting the values on the back of his slide rule and calculating the rate of rise. After it had stabilized, Fermi told Weil to move the rod out another six inches. Again the neutron intensity increased and leveled off. The pile was still subcritical. Fermi had again been busy with his little slide rule and seemed very pleased with the results of his calculations. Every time the intensity leveled off, it was at the values he had anticipated for that position of the control rod. He moved the rod out another six inches. After it had stabilized this time, the neutron intensity in the pile had reached an intensity that was too high for some of the instruments, and, as in other experiments, a few of the instruments were no longer in their linear range. We wanted to take some time to rectify the situation and to modify the operating range of some of the instruments.

After the instrumentation was reset, Fermi told Weil to remove the rod another six inches. The pile was still subcritical. The intensity was increasing slowly—when suddenly there was a very loud crash! The safety rod, ZIP, had been automatically released. Its relay had been activated by an ionization chamber because the intensity had exceeded the arbitrary level at which it had been set. It was 11:30 am, and Fermi said, "I'm hungry. Let's go to lunch." The other rods were put into the pile and locked.

I ate at the cafeteria at the University Commons. After all these months, we had become adjusted to not discussing our work outside the laboratory, so there could be no discussion over lunch of the morning's events.

It is important to understand what Fermi was doing in the morning by making these measurements. The most important thing was to establish the position of the control rod at which the pile would become self-sustaining, that is, the critical point. The next thing was to establish how fast the intensity would rise if he moved the rod beyond that point. He could establish the critical point by two methods: to extrapolate from the intensity measurements; or to note that the exponential rate of rise had become zero, indicating the critical condition. The rate of rise would become longer and longer approach-

Norman Hilberry (left) and Leo Szilard at West Stands.

Eugene Wigner took out a bottle of Chianti and presented it to Fermi. We each had a small amount in a paper cup.

ing critical, then after passing through the critical it would start to get shorter again. Fermi was determining that both these methods were giving the same result. There had been some uncertainty in the value of one of the constants used in the formulas for the exponential rate of rise. From the first couple of measurements, he obtained the value of the constant, which was what he had hoped it would be. Continuing measurements had been to establish that he could predict precisely what would happen when he moved the control rod a fixed distance. That is, he could predict both the rate at which the intensity would increase with time and the intensity at which the pile would stabilize. Being able to make reliable predictions indicates not only a quantitative understanding of the physics, but also the reliability of the detectors and instrumentation.

So, in the morning Fermi had established that he had control, that he could predict the reaction of the pile, and that the instrumentation was reliable at the intensities at which he needed to make measurements. When we returned after lunch, Compton, who had not been there in the morning, joined us, bringing Crawford Greenwalt of the DuPont company with him. Compton had been meeting with an important committee that had just stopped over in Chicago for a few hours. Volney Wilson had phoned him in the morning to say that there was very little room on the balcony.

In the afternoon there were changes in what some of the younger people were doing. Herb Anderson, Bill Sturm, and Leona Woods were recording the readings from instruments. Somehow we got a public address system and Bill Overbeck was set up to call out the neutron counts. Fermi was set up to watch the pen on the recording chart which was attached to a neutron detector. I took my turn on the elevator.

Except for the one hand-controlled rod, all the other rods were again removed. Fermi asked for the last hand-controlled rod to be set at one of the positions where it had been in the morning. He checked the intensity and the rate of rise and the functioning of the instruments. The values were the same as they had been during the morning when the control rod was at that same position. He then asked George Weil to set the rod where it had been before we went to lunch.

The trace on the paper on which the neutron intensity was being recorded showed the intensity rising slowly, at the rate that Fermi expected. The intensity would have levelled off after an appreciable length of time. The pile was getting close to critical. Fermi measured the changes in the rate of rise for a while, then asked that ZIP be put in to bring down the intensity. He told George Weil, "This time, take the control rod out twelve inches." After the control rod was set, the ZIP rod was removed from the pile, and Fermi said to Compton, who was standing at his side, "This is going to do it. Now it will become self-sustaining. The trace will climb and continue to climb; it will not level off." Fermi computed the rate of rise after a minute. After another minute, he computed it again. After three minutes, he calculated the rate of rise again, and it was staying the same. The pile was functioning exactly as he had expected. I have heard that at this point he broke into a big, cheerful smile. He put away his slide rule and announced, "The reaction is self-sustaining."

Fermi let the activity of the pile increase and watched the pen. It continued to rise as it should, and the intensity was not leveling off. At 3:53, Fermi told Zinn to put ZIP in. The radiation and the neutron intensity and the counting rates all decreased almost instantaneously. We had built the pile, and Fermi had established that we could get a self-sustaining nuclear reaction that we could control in a very predictable manner.

Eugene Wigner had a paper bag with him that I had not noticed. He took out a bottle of Chianti and presented it to Fermi. We each had a small amount in a paper cup and drank silently, looking at Fermi. Someone told Fermi to sign the wrapping on the bottle. After he did so, he passed it around, and we all signed it, except Wigner.

People turned off the electric power on the instruments and slowly left. I think Bob Nobles and I got the cadmium sulfate bottles as far away from the pile as we could.

After all the others had left, I stood there just looking at the pile. My mind was wandering over all the machining, pressing, stacking that the gang of us had done. I recalled some of the minor incidents that could have turned into major delays or disasters. I had a tremendous feeling of accomplishment.

Then my mind wandered in the wrong direction—I started thinking about the work that lay ahead. So I went around and checked that all of the rods were locked in place, that all the power supplies were turned off. I hung up the Chianti bottle on the wall and threw away the cups. I then went home to my room to sleep for twelve hours. ☐

1. Herbert L. Anderson, "Assisting Fermi," in *All in Our Time* (Chicago: Educational Foundation for Nuclear Science, 1974).
2. As well as Anderson and Zinn, those involved were: H. Agnew, A.C. Graves, D.L. Hill, P.G. Koontz, H. Kubitschek, H. Lichtenberger, G. Miller, R. Nobles, W. Nyer, L. Sayvetz, L. Seven, W. Sturm, A. Wattenberg, and G. Weil.
3. Enrico Fermi, *Collected Papers*, 2 vol., Emilio Segre, ed. (Chicago: University of Chicago Press, 1965).
4. Those involved were: H.M. Barton, Jr., T. Brill, R. J. Fox, S. Fox, D. Froman, W. H. Hinch, W.P. Overbeck, J.H. Parsons, G.S. Pawlicki, L. Slotin, R.J. Watts, M.H. Wilkening, and V.C. Wilson.

NORMAN F. RAMSEY

August 1945: the B-29 flight logs

In September of 1945, Norman F. Ramsey wrote the "History of Project A," the project coordinating all activities concerned with the first combat use of nuclear weapons. (Captain W.S. Parsons was officer in charge of Project A, and Ramsey was his deputy for scientific and technical matters.) The report, declassified in 1973, includes many details and is too long to reproduce in its entirety. Following is Ramsey's account of the atomic bombings themselves.

On 26 July the uranium-235 projectile for the Little Boy was delivered by the cruiser *Indianapolis*. The uranium-235 target insert arrived in three separate parts in three otherwise empty Air Transport Command C-54s during the evening of 28 to 29 July. All three had arrived by 0200 29 July. Since the earliest date previously discussed for combat delivery of the Fat Man was 5 August (at one time the official date was 15 August), [Captain W.S.] Parsons and [N. F.] Ramsey cabled General [Leslie] Groves for permission to drop the first active unit perhaps as early as 1 August, with 2 August being more probable since the weather was forecast to be bad on 1 August.

Although the active unit, No. L11 was completely ready in plenty of time for a 2 August delivery, the weather was not. The first, second, third and fourth of August were spent in impatient waiting for good weather. Finally on the morning of 5 August we received word that the weather should be good on 6 August. At 1400 on 5 August General [Curtis] Lemay officially confirmed that the mission would take place on 6 August.

The Little Boy was loaded on its transporting trailer at 1400 5 August and with an accompanying battery of official photographers under 9-2 supervision was taken to the loading pit. The B-29 was backed over the pit at 1500 and the unit was loaded shortly thereafter. The aircraft was then taxied to its hard stand where final testing of the unit was completed. By 1800 all was ready. Between then and take off the aircraft was under continuous watch both from a military guard and from representatives of the key technical groups.

Final briefing was at 0000 of 6 August. Following this and an early breakfast the crews assembled at their aircraft. There amid brilliant floodlights their pictures were taken and retaken by still and motion picture photographers as if for a Hollywood premier. For this mission Col[onel] P.W. Tibbets was pilot of the B-29, named the *Enola Gay* which carried the bomb, Major Thomas Ferebee was bombardier, Captain W.S. Parsons was bomb commander, and L[ieutenan]t Horris Jepson was electronics test officer for the bomb. L[uis] Alvarez, Bernard Waldman, Harold Agnew and Larry Johnston rode in the accompanying observation aircraft.

The progress of the mission is best decribed in the log which Capt[ain] Parsons kept during the flight:

Luis Alvarez with a B-29, bomber of the type used to attack Hiroshima and Nagasaki, on Tinian.

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6 August 1945

0245	*Take off*
0300	*Started final loading of gun*
0315	*Finished loading*
0605	*Headed for Empire from Iwo*
0730	*Red plugs in (these plugs armed the bomb so it would detonate if released)*
0741	*Started climb. Weather report received that weather over primary and tertiary targets was good but not over secondary target*
0838	*Leveled off at 32,700 feet*
0847	*All Archies (electronic fuses) tested to be O.K.*
0904	*Course west*
0909	*Target (Hiroshima) in sight*
0915½	*Dropped bomb (originally scheduled time was 0915) Flash followed by two slaps on plane. Huge cloud*
1000	*Still in sight of cloud which must be over 40,000 feet high*
1003	*Fighter reported*
1041	*Lost sight of cloud 363 miles from Hiroshima with the aircraft being 26,000 feet high*

The crews of the strike and observation aircraft reported that 5 minutes after release a low 3 mile diameter dark grey cloud hung over the center of Hiroshima, out of the center of this a white column of smoke rose to a height of 35,000 feet with the top of the cloud being considerably enlarged.

Four hours after the strike photo-reconnaissance planes found that most of the city of Hiroshima was still obscured by the cloud created by the

A Hiroshima temple in which hundreds died after the atomic bomb was dropped. This drawing by a survivor is one of many included in *The Unforgettable Fire*, the first U.S. exhibition of these drawings, recently on display at the Peace Museum, Chicago.

explosion although fires could be seen around the edges. However, the following day excellent pictures were obtained which showed the tremendous magnitude of the power of a single atomic bomb, which completely destroyed 60 percent of the city of Hiroshima.

The first Fat Man with active material, unit F31, was originally scheduled for dropping on 11 August local time (at one time the schedule called for 20 August). However, by 7 August it became apparent that the schedule could be advanced to 10 August. When [Captain] Parsons and Ramsey proposed this change to [Colonel] Tibbets he expressed regret that the schedule could not be advanced two days instead of only one since good weather was forecast for 9 August and the five succeeding days were expected to be bad. It was finally agreed that Project A would try to be ready for 9 August provided it was understood by all concerned that the advancement of the date by two full days introduced a large measure of uncertainty into the probability of our meeting such a drastically revised schedule. However, all went well with the assembly and by 2200 of 8 August the unit was loaded and fully checked.

The strike plane and two observing planes took off at 0347 local time on 9 August. Major C.W. Sweeney was pilot of the strike ship, Capt[ain] K.K. Beahan was bombardier, Com[mander] F.L. Ashworth was bomb commander, and L[ieutenan]t Philip Barnes was electronics test officer.

SECRET CLASSIFICATION CANCELLED PER DOC REVIEW JAN. 1973

This mission was as eventful as the Hiroshima mission was operationally routine.

Due to bad weather between Tinian and Iwo Jima a preliminary rendezvous was not planned for the three aircraft at Iwo Jima and instead the briefed route to the empire was from Tinian direct to Yakashima on Kyushu. The briefed mark cruising altitude was 17,000 feet. Com[mande]r Ashworth's log for the trip is as follows:

9 August 1945

0347 *Take off*
0400 *Changed green plugs to red prior to pressurizing*
0500 *Charged detonator condensers to test leakage. Satisfactory*
0915? *Arrived rendezvous point at Yakashima and circled awaiting accompanying aircraft*
0920 *One B-29 sighted and joined in formation.*
0950 *Departed from Yakashima proceeding to primary target Kokura having failed to rendezvous with second B-29. The weather reports received by radio indicated good weather at Kokura (3/10 low clouds, no intermediate or high clouds, and forecast improving conditions) of The weather reports for Nagasaki were good but increasing cloudiness was forecast. For this reason the primary target was selected.*
1044 *Arrived initial point and started bombing run on target. Target was obscured by heavy ground haze and smoke. Two additional runs were made hoping that the target might be picked up after closer observation. However at no time was the aiming point seen. It was then decided to proceed to Nagasaki after approximately 45 minutes had been spent in the target area.*
1150 *Arrived in Nagasaki target area. Approach to target was entirely by radar. At 1158 the bomb was dropped after a twenty second visual bombing run. The bomb functioned normally in all respects.*
1205 *Departed for Okinawa after having circled smoke column. Lack of available gasoline caused by an inoperative bomb bay tank booster pump forced decision to land at Okinawa before returning to Tinian*
1351 *Landed at Yontan Field, Okinawa*
1706 *Departed Okinawa for Tinian*
2245 *Landed at Tinian*

Due to bad weather, good photo reconnaissance pictures were not obtained until almost a week after the Nagasaki mission. These showed that the bomb detonated somewhat north of the Mitsubishi Steel and Arms Works. All other factories and buildings on the Urakami River from the Nakajima Gawa River through the Mitsubishi Urakami Ordnance Plant were destroyed. The distance from the northernmost factory that was destroyed to the southern boundary of complete destruction was about three miles and damage might have occurred north of the Urakami Ordnance Plant if any buildings had been there. Although only 44 percent of the city was destroyed by the official record, this was due to the unfavorable shape of the city and not to the location of the bomb detonation.

On the day following the Nagasaki mission, the Japanese initiated surrender negotiations. Consequently further activity in preparing active units was suspended. ☐

Nagasaki
September 14, 1945

"The atomic bomb damage is a story in itself. Rubble and ruins I know and can look upon without any particular emotion. This is indescribably different. As you enter the harbor and look at the buildings from a distance they appear to be intact. As you come closer you see that the houses are windowless. The shingles on the roofs have the appearance of a moulting hen. The ridge rafters are broken, allowing the roofs to sag in the middle. Many of the older houses have just collapsed like a tired beast of burden which has fallen from exhaustion. A smell of death and corruption pervades the place, ranging from the ordinary carrion smell to somewhat subtler stenches with strong overtones of ammonia (decomposing nitrogenous matter, I suppose). The general impression, which transcends those derived from the evidence of our physical senses, is one of deadness, the absolute essence of death in the sense of finality without hope of resurrection. And all this is not localized. It's everywhere, and nothing has escaped its touch. In most ruined cities you can bury the dead, clean up the rubble, rebuild the houses and have a living city again. One feels that it is not so here. Like the ancient Sodom and Gomorrah, its site has been sown with salt and ichabod is written over its gates.

Tons of newsprint will be devoted to the things written about this place, and all of it far better written than what I can write, but the real thing can't be put into words. Therefore I shall stop trying."

CAPTAIN WILLIAM C. BRYSON (U.S.N.)

Excerpt from a letter written by Captain Bryson to his wife, Tibee, when, as operations officer on the staff of Admiral Frank G. Fahrion, he was sent to Nagasaki to remove U.S. POWs before the occupation troops arrived. His widow has given permission for its publication.

From *The Winning Weapon* GREGG HERKEN

Epilogue

During fall 1945, at the peak of public controversy over American policy on the atomic bomb, columnist Walter Lippmann wrote of the dangers posed by an irrational attachment to an atomic secret. "[I]f the secret cannot be kept," Lippmann warned, "it is unnecessary to argue whether it ought to be kept. . . . Moreover, it would be in the highest degree dangerous to suppose we were keeping the secret if in fact we were not. . . . For that could only give us, as it has already given many, a false sense of security and a false sense of our own power. . . ."

Nearly five years later, when that warning seemed prophetic and Truman's pending decision on the hydrogen bomb had reopened the debate on American power and security, Lippmann returned to the theme he had raised in the shadow of Hiroshima. Writing to physicist Chester Barnard, he reflected upon the extent to which American policy on the atomic bomb had been influenced—or even determined—by the mistaken assumption that the nation's nuclear supremacy would be enduring: "[I]f we had known in 1948 how far advanced the Soviet Union was toward the production of bombs . . . would [we] not have then changed our tactics and attitudes?"

Barnard, then completing a *Scientific American* article opposing the "Super," concurred strongly in his reply:

> "The fallacy which governed the tactics and attitudes of that time was in the minds of the people and in the country and of the representatives in Congress, particularly in the Senate. There there was a belief, a most deadly illusion, that we could retain a monopoly of the facilities and the knowledge for the production of fissionable materials. . . . Hence if you should say of the tactics of the negotiations and the attitude about them that [they were based] upon a popular fallacy as to the indefinite continuance of the monopoly, I should agree."

That expectation of continuing nuclear supremacy which Barnard termed "a popular fallacy" and "a most deadly illusion" was, in fact, a cause of the false sense of security and power which had been so suddenly and dramatically undermined by 1950. There can be no question of the attraction that the prospect of a lasting atomic advantage had for many Americans. It was this allure which blinded such initial critics of the policy of monopoly as James Conant and Vannevar Bush to the fatal flaw in that policy. It later caused others, such as Secretary of State James Byrnes, Bernard Baruch, and General Leslie Groves, to ignore evidence that the atomic monopoly was not preclusive and was rapidly slipping away. Ultimately, it locked the Truman Administration into a policy of trying to outrace the Russians in the development of new and more terrible means of destruction.

Seeking a cause many years after Hiroshima for the fundamental miscalculation in American estimates concerning the Russian atomic bomb, Leo Szilard—the scientist to whom Byrnes had given Groves' assurance that the Russians were without uranium—mused: "If you are an expert, you believe that you are in possession of the truth, and since you know so much, you are unwilling to make allowances for unforeseen developments." Of course, the peculiar insularity that the monopoly fostered was common not only to civilian and military planners in the Truman Administration, but to the public as well. Wishful thinking as much as unforeseen events contributed to the error. Even those citizens who had accustomed themselves to the idea that American nuclear supremacy would be short-lived, one pollster discovered, "continued to talk entirely in terms of the monopoly" when discussing the future.

There was still another "deadly illusion" that was not dispelled with the collapse of the policy of monopoly in 1949, as shown by Truman's decision on the super-bomb and by the evolution of NSC-68. This was the fallacious assumption concerning the utility of nuclear weapons: their supposed efficaciousness in diplomacy, and their alleged capacity to avert military confrontations. It survives as myth to the present day, although the experience of American foreign policy since 1950 has only tended to confirm that more nearly the opposite of this tenet is true. Nuclear weapons some 35 years after the destruction of Hiroshima have yet to convincingly prove themselves an asset to diplomacy—even though the United States has variously held superiority, supremacy, and a monopoly in such weapons during that time.

This is not to argue that an altogether different policy on the bomb than that adopted by the Truman Administration would have avoided the

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Cold War or could have lessened subsequent Soviet-American enmity. Apart from a shared interest in survival, Russia and the West have certainly not bridged their differences; nor does the gap between them seem to have perceptibly narrowed. But simply because a more cooperative attempt was never attempted, its possible results cannot be known. It would be facile to claim, moreover, that the policy of monopoly has been vindicated in the light of subsequent events. Rather, by straining relations with traditional allies like Britain and by giving impetus to the nuclear arms race, that policy had the effect of intensifying the Cold War. . . .

The deadly illusion of enduring nuclear supremacy also had its price at home. Indeed, it may be that the more serious and lasting impact of America's brief period of nuclear hegemony was not upon foreign policy, but at home, by creating an atmosphere of failed expectations and anxiety. The atomic secret's effect upon the domestic Cold War was to give credibility to the bogus espionage scare that began in 1946 and that was revived—on equally dubious grounds—during the 1948 election campaign. The documented evidence of spying in the cases of Klaus Fuchs and Donald Maclean,* the conviction for perjury of Alger Hiss, and the 1950 espionage trial of Ethel and Julius Rosenberg all lent a seeming credence to what has become a persistent myth of the nuclear era: that the atomic secret was a tangible commodity stolen by spies, rather than a product of arrogance and misconception.

The actual damage done to American security by Soviet atomic espionage cannot be accurately assessed even today. But probably of far greater value to the Russians than any technical data they may have gained by spying was their certain knowledge that Western assumptions concerning "primitive" Soviet technology and the preclusive nature of the West's atomic raw materials monopoly were false. The harm done by Soviet spies was certainly far less significant than the hysteria induced by the fear of such spying.

Conducted against the backdrop of the Korean War and in the midst of resurgent "atom spy" allegations, the Rosenbergs' trial for the theft of the atomic secret—still branded the "crime of the century" by the FBI—represents perhaps the culminating domestic event of the era begun with Truman's announcement of the atomic bomb dropped upon Hiroshima. But it was hardly the last episode to show the political fallout of the unexpectedly early Soviet atomic test.

One of the compounded ironies of this era is that the myth of the atomic secret would be exposed some months after the Rosenbergs' execution by the man most responsible for creating and promoting that myth—General Leslie Groves. Testifying at the 1954 Atomic Energy Commission investigation into Robert Oppenheimer, whose loyalty was declared suspect, Groves acknowledged that "reliance on what the Russians could or could not do was based on primarily the supplies of materials which I felt would be available to them." He had overlooked, Groves admitted, Soviet access to uranium ore in Russian-occupied eastern Germany after the war. That Groves' belated and unrepentant admission of error went unnoticed in the three-week and thousand-page testimony of the security hearings is perhaps no more remarkable, given the climate of opinion in 1954, than that the charge of disloyalty against Oppenheimer would be upheld.

Oppenheimer—as much as Hiss, the Rosenbergs, and the other victims or villains of the Cold War—became a symbol of the political era created by the atomic bomb. Truman, at various times either a defender or a detractor of the scientist, seemed to recognize this in a 1952 letter to Atomic Energy Commissioner Gordon Dean. "I feel as you do that Dr. Oppenheimer is an honest man," the President wrote. "In this day of the character assassination and unjustified smear tactics it seems that good men are made to suffer unnecessarily. . . . I hope we will live through the smear age without destroying our Bill of Rights."

The fact that what Truman aptly dubbed "the smear age" began with the end of American nuclear hegemony was hardly coincidental. That unfortunate era was in substantial part the inevitable result of the false sense of security and power which Lippmann cautioned against in the aftermath of Hiroshima, but that misplaced faith in a winning weapon had brought about. As Lippmann was aware in 1950, American policy on the atomic bomb in the Cold War had come full circle. Reliance upon a winning weapon had not only been the inspiration for that policy, but was its ironic legacy as well—the legacy of an arms race now renewed with even greater intensity; and of a frantic search for traitors at home and new allies abroad. In these alone, where security itself would prove elusive, the illusion of security remained. □

*Maclean's spying—like that of Fuchs—was publicly portrayed as much more damaging to American interests than classified official accounts of the time indicated. Hence a top-secret "damage assessment" concerning Maclean, sent to the FBI by the Atomic Energy Commission shortly after his defection, concluded that "only a rough order-of-magnitude estimate of fissionable material could be derived from [his] information." Maclean's knowledge might actually have served to deceive the Russians, the authors of the report noted, for "we do not believe that the information available to Maclean in 1947-8 would now be of any appreciable aid to the Soviet Union because of the changes in the rate and scale of the U.S. program which have taken place in the intervening years . . . the results would be widely misleading as to our present or prospective position in fissionable materials. . . ."

ALICE KIMBALL SMITH

Scientists and public issues

The scientists' movement early acquired an aura of importance not least because of the magnitude and global ramifications of the problem with which it tried to grapple. A certain epic quality attaches to any experience by which men are so stirred as to be wrenched from their accustomed patterns of behavior, and in addition this particular experience stemmed from a decision in which elements of immediate and ultimate good, of right and wrong and expediency appeared in all their baffling complexity—a decision that threw into sharp relief man's unevenly developed powers over the realms of matter and spirit. Mankind now faced a common destiny of destruction as represented once before by the universal story of the flood. And the scientists' own world was abruptly enlarged. Science, once the exclusive province of a dedicated few, seemed suddenly to be of interest to everybody, and just as suddenly scientists found that they themselves possessed a hitherto unrecognized aptitude for practical things, whether it was making bombs or persuading legislators to espouse a particular course of action.

There remain, however, many questions of the utmost relevance to the place of science and scientists in our political society that are at least illuminated if not finally answered by developments of 1945 and 1946, for it was a period in which practical aspects of the scientist's dilemma were amply demonstrated. If he devotes himself intensively to public affairs, how can he maintain the scientific competence on which his usefulness rests? When only a few individuals have access to all the technical data needed to decide a given point, how can collective scientific wisdom play its part, and how can it be effectively expressed when the natural spokesmen for the profession become confidential advisers to government? What happens to the authority of science when scientists do not agree on the answer to a given question? And where, if at all, do they draw the line between their roles as scientists and citizens? And there are questions of more specific relevance to the scientists' movement—its effect upon postwar policy and upon the attitude of scientists toward participation in public affairs—upon which one must render some account even though the answers cannot be quantitative or complete. □

From *A Peril and a Hope*, Alice K. Smith (Chicago and London: University of Chicago Press, 1965), pp. vi-vii.

"We know the facts of atomic energy because we worked on the bomb," said scientists in the late summer of 1945. A missionary impulse gripped hundreds of young U.S. scientists in the months following Hiroshima. They organized to spread a warning first formulated in the spring of 1945 at the Chicago Manhattan Project's Metallurgical Laboratory by a committee chaired by Nobel Laureate James Franck. The Franck report said of the yet untested weapon that it could conceive no defense against it; that there is no basic secret and therefore no monopoly; that far more destructive weapons will soon be available; that only an international control agreement can prevent an unprecedented arms race; that dropping the bomb on Japan without warning will undermine the confidence essential to such an agreement; and that therefore the bomb should be demonstrated to the Japanese government in an uninhabited area, giving opportunity for surrender before a city is bombed.

Not many scientists knew of the demonstration proposal. Not all who knew concurred, nor do they agree today on its merits. But when the war ended and restrictions on communications were relaxed, there was widespread acceptance of its basic premises. No defense, no secret, no monopoly, therefore international control, became the rallying cry of the organizations which formed in late August 1945 at major Project sites. Chicago had ready access to a large Midwest audience. Oak Ridge was isolated in the Tennessee hills, but unofficial emissaries, using long deferred vacations, visited New York and Washington and established instant friendships with newsmen, peace groups and congressional aides.

The atomic energy message that President Harry S. Truman sent to the Congress on October 3, 1945 endorsed the principle of international control and a domestic policy which would prevent private exploitation; but a War Department bill, introduced into both houses of Congress the following day, appeared incompatible with the spirit of the President's message. It placed domestic programs in the hands of a part-time commission which could include military representatives, and its militaristic tone seemed calculated to destroy the credibility of a U.S. offer to share control of the bomb. Manhattan Project scientists had had their fill of Army su-

James Franck

pervision, and hundreds of their colleagues outside the Project agreed that the bill's secrecy provisions would both jeopardize national security by stifling research and blast any hope of international control by preventing exchange of data. Opposition was spearheaded by prominent Manhattan Project scientists—Leo Szilard and Harold Urey, names that made headlines—but what really excited reporters and commentators was the spectacle of young men from the site organizations testifying at committee hearings, meeting the press and making eloquent speeches about the threat to national security and world peace.

At the end of a critical week in October 1945, Raymond Swing, a liberal oracle of the day, reported to his national radio audience on what he called "science week in Washington":

"In the wake of the atomic bomb which they created now come the scientists, . . . marching upon the stage of public affairs, and, for the first time in their lives, taking a lead in guiding the nation. . . . They have proved to be as impressive a group of men as ever came to modern Washington."

A natural impulse toward healing prompted many people of good will at this period to support some form of international collaboration. For young scientists who had worked in secrecy on the bomb there was something both exhilarating and salutary in continued collective activity that could be openly avowed and freely discussed.

By January 1946 the coalition of Manhattan Project site groups had expanded into the Federation of American Scientists—the *pater familias* of the numerous organizations which have sought in one way or another to convey the stern message of the Franck report. The War Department bill stalled in committee, and for the first half of 1946 the Federation lobbied on behalf of substitute legislation under which a civilian U.S. Atomic Energy Commission took office on January 1, 1947. After mid-June, when the United States' control plan was presented to the U.N. Atomic Energy Commission, the Federation kept an anxious eye on negotiations—applauding the conclusion, reached in September by a subcommittee of Soviet and Western scientists, that inspection and control were technically feasible, then watching with dismay as talks foundered on political issues.

As hope of international agreement diminished, so did participation in the Federation's political and educational activities. By 1950 membership had dropped from over 3,000 to a third of that number. Local groups gradually disbanded and, with a hundred or so active members, the Washington office served as a kind of watchdog over matters of concern to scientists until new issues attracted new members in the late 1960s.

The other direct link with the postwar missionary fervor is the *Bulletin of the Atomic Scientists*, first published in December 1945 as the newsletter of the Atomic Scientists of Chicago, founded the previous August. Its editors, physicist Hyman H. Goldsmith and biochemist Eugene Rabinowitch, had foreseen the need of a

Harold Urey

forum to explore problems created by the bomb. Within a matter of weeks the *Bulletin* began printing official documents and articles relating to atomic energy, thus providing other journalists with authoritative information and future historians with a unique record of the changing relationship of science, politics and society. Goldsmith died in 1949. With unflagging dedication to the cause of atomic education Rabinowitch continued as editor until his death in 1973.

Elitism was not yet a household word in 1945, but the concept was implicit in the claim of the so-called atomic scientists that they possessed certain information essential to sound policy decisions. On this basis they captured the attention of reporters and legislators. At the same time, the young crusaders acknowledged their lack of political experience, and their statements of purpose included the resolution "to study and inform." In a spirit of humility that was not easy to maintain amidst the public acclaim showered upon fabricators of the weapon that had ended the war, they sought advice from social and political scientists, government officials, clergymen, labor leaders and businessmen. If this tactic did not elicit much practical wisdom, it did give access to large and attentive audiences to

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which scientists explained why international control was essential.

By November 1945, their still embryonic organization had been instrumental in forming a National Committee on Atomic Information composed of 49 groups with a combined membership of over ten million. In the next two years this committee distributed vast quantities of literature about domestic legislation and international negotiations. Thus the Federation of American Scientists tried both to enlist the support of an influential elite and to develop a mass movement. Pressure from voters was probably crucial in passage of the Atomic Energy Act of 1946, but the international control issue disintegrated without opportunity for voter input.

In retrospect, it is clear that behind-the-scenes pressure by individual scientists was also very important in the autumn of 1945. Vannevar Bush, James B. Conant and Robert Oppenheimer played significant roles in the adoption of international control as official United States policy. On the issue of domestic control by civilians, Leo Szilard ferreted out allies within the Truman Administration whose opposition to the War Department bill was critical in shelving it.

By the early 1950s the Federation of American Scientists had abandoned hope of creating a mass movement. It had become, as it has remained, an organization composed principally of scientists dedicated to influencing those who make policy. Other movements that attracted scientists in the 1950s were also elitist in that they tried to capitalize on their special knowledge. Chemist Linus Pauling's petition to stop nuclear testing was signed by thousands of scientists around the world.

The Atoms for Peace Conference, held in Geneva in August 1955 under the sponsorship of President Dwight D. Eisenhower, brought scientists from East and West together again. The Pugwash Conferences on Science and World Affairs were founded in the hope that specialists could influence their governments. Scientists who refused to do research with direct military application organized the International Society for Responsibility in Science, and in 1960 the American Association for the Advancement of Science, through its Committee on Science and Human Welfare, declared that "scientists bear a serious and immediate responsibility to help mediate the effects of scientific progress." Social and political caucuses within professional societies were a further recognition that scientists had both influence and responsibilities outside their own fields.

A renewed disposition to seek support from the lay public was evident in organizations founded in the 1960s. The purpose of the Council for a Livable World, established by Leo Szilard in 1962, was ostensibly to elect senators who would vote for arms control. But from the beginning the Council solicited funds from donors outside science, and so long as its successful candidates retained their seats there was an educational feedback to voters. The Union of Concerned Scientists in 1969, publicizing the hazards of nuclear power development, encouraged citizens to become involved and recruited thousands of non-scientist members while continuing to stress the importance of the technical data provided by its experts.

The strategy of the scientists who worked so hard after the war to promote international control was strongly influenced by the liberal tenet that the informed citizen would vote the right way. Now more hope than conviction, this view still animates leaders in what appears to be the most promising upsurge of opposition to continued nuclear escalation since Hiroshima. Over the years I have been in close touch with many participants in efforts to control the atom—at Los Alamos during and immediately following the war, in Chicago as assistant editor of the *Bulletin*, and later as a historian of the postwar movement.

In 1981 I talked again with a few of the participants. Those who witnessed the first nuclear explosion on the New Mexico desert on July 16, 1945 have never forgotten what they saw—the light, the cloud, the sound, then the awesome stillness and the finality of the desolation spread across the barren earth around them. But they also remember the slender hope profferred by Robert Oppenheimer in his farewell speech to Los Alamos colleagues in October 1945—that because the release of atomic energy constituted so unprecedented a peril it also provided a hope that nations might at last be forced to live in peace.[1]

Philip Morrison

The young physicist entrusted with transporting the precious plutonium core of the explosive device from Los Alamos to the test site was Philip Morrison, now Institute Professor at MIT. Among scientists Morrison is known as a brilliant theorist with a keen analytical mind and an incredible capacity for acquiring and retaining information. He made significant contributions to nuclear physics and the understanding of cosmic rays, and now concerns himself primarily with

high energy astrophysics, quasars, supernova explosions and the origins of the cosmos. To a wider public Morrison is familiar as an expounder of science on public television and a stimulating reviewer of books for *Scientific American*. Drawing on a vast store of facts and anecdotes, he immediately dominates a lecture platform, and his elegant language, whether he talks about quasars, termites or nuclear weapons, is infused with a kind of fire that holds an audience spellbound.

Three years after obtaining his doctorate under Robert Oppenheimer at Berkeley, Morrison joined the Manhattan Project, first in Chicago, then at Los Alamos. "I'd been a college radical in the thirties," he recently told a class at Harvard, "but I was also a patriotic American and I wanted to help win the war." Morrison was a member of the team that assembled the Nagasaki plutonium bomb on the island of Tinian, and after Japan surrendered he flew over the devastated cities of Honshu. They all looked the same, he told a New Mexico radio audience on his return—a series of rust-red circles produced by hundreds of fire bombs. Hiroshima was also a rust-red circle, but its telltale scar was the work of one bomb. That, said Morrison succinctly, was the new thing.

Morrison helped organize the Association of Los Alamos Scientists, then the national coalition of atomic scientists, contributing trenchant phrases to the press releases that put him and other political neophytes in the Washington limelight in the autumn of 1945. He made an effective witness before congressional committees. He helped prepare classified reports on how to detect laboratories, test sites and assembly plants—essential background for the committee that drafted the U.S. international control plan in early 1946. After the plan was presented to the U.N. Atomic Energy Commission in June, Morrison served for a time as consultant to the U.S. representative, financier Bernard Baruch, although he regarded Baruch's appointment as confirmation of the Cold War spirit in which the U.S. government was approaching the negotiations.

The Federation of American Scientists remained the principal channel through which Morrison has expressed his concern about nuclear weapons, serving as council member and sometime chairman. "The FAS is not a perfect instrument," Morrison explains, "but it represents a self-help effort on the part of people who can knowledgeably criticize official policy." Seeing himself as temperamentally an outsider, he likes to work this way rather than through the committees and consultancies that drew so many of his friends into government. In addition to writing articles and lecturing, Morrison has explored alternatives to the nuclear arms race, most recently as co-author of *The Price of Defense: A New Strategy for Military Spending*, published by The Boston Study Group in 1979. The thoroughly documented study concluded that as of 1978 an expenditure of $73 billion could have provided greater security than the high technology that had cost $120 billion. The Pentagon found *The Price of Defense* very useful and bought a lot of copies, but for Morrison the overall effect was disappointing. While the recommendations were widely applauded by those who agreed, they were generally ignored by those who didn't, so the debate never developed.

Bernard T. Feld, a slightly younger contemporary of Morrison, was still a graduate student at Columbia when he became Leo Szilard's assistant in experiments with Enrico Fermi that led to the first self-sustaining nuclear chain reaction on December 2, 1942.

A Petition to the President of the United States

"Discoveries of which the people of the United States are not aware may affect the welfare of the nation in the near future. The liberation of atomic power which has been achieved places atomic bombs in the hands of the Army. It places in your hands, as Commander-in-Chief, the fateful decision whether or not to sanction the use of such bombs in the present phase of the war against Japan. . . .

"In view of the foregoing, we, the undersigned, respectfully petition: first, that you exercise your power as Commander-in-Chief, to rule that the United States shall not resort to the use of atomic bombs in this war unless the terms which will be imposed upon Japan have been made public in detail and Japan knowing these terms has refused to surrender; second, that in such an event the question of whether or not to use atomic bombs be decided by you in light of the considerations presented in this petition as well as all the other moral responsibilities which are involved."

So concluded Leo Szilard's petition to President Truman, dated July 17, 1945. Circulated at the Metallurgical Laboratory in Chicago, the appeal was signed by 70 scientists. Szilard's petition was re-written when it reached Clinton Laboratory in Oak Ridge, Tennessee, where two versions were signed by a total of 85 scientists before circulation was stopped by military authorities. When the petition reached Los Alamos, J. Robert Oppenheimer refused to permit it to circulate. Apparently, none of the petitions ever reached the President.*

*See Alice Smith, *A Peril and a Hope* (Chicago and London: The University of Chicago Press, 1965), pp. 53-56.

Public pressure did swing the balance in favor of the Partial Nuclear Test Ban Treaty of 1963.

He completed the doctorate while working for the Manhattan Project in Chicago and Los Alamos. In 1946 Feld did a stint as volunteer in the Federation of American Scientists' Washington office, preparing testimony for Senate hearings, talking to newsmen and grinding the mimeograph machine.

Immediately after the war Leo Szilard, Eugene Rabinowitch and other senior scientists who had known what it was like to be part of an international community of science began talking about ways of re-establishing contact in order to discuss problems created by the bomb. The Federation had tried to do this with committees of correspondence, but in 1957 a more effective exchange began through the Pugwash Conferences on Science and World Affairs, founded in the expectation that views expressed by participants, especially when there was consensus, would be conveyed to their respective governments. Feld soon took an active part in these annual or semi-annual gatherings—held in Canada, India, Western Europe, the Soviet Union and the United States—at which scientists and specialists in other fields discuss biological warfare, nuclear test bans, accidental war, problems of the Third World and other topics. From 1974 to 1977 Feld served as secretary general at the Pugwash London headquarters.

Meanwhile, Feld had helped Leo Szilard to establish the Washington-based Council for a Livable World to raise funds for peace-minded candidates and to set up a technical information service for members of Congress. The Council, Feld reflects, never got anywhere near its goal, that is, a Senate majority ready to vote for substantial arms reduction, but its funds probably elected seven or eight members who made a difference, notably George McGovern.

Over the years Feld contributed articles to the *Bulletin*, and after Rabinowitch died in 1973 Feld's name often appeared on guest editorials. In November 1975 he became editor-in-chief. While steadily expanding its coverage the *Bulletin* retained the historically significant "atomic scientists" in its title. Feld believes the magazine provides an important forum for expressing the concerns of a broad segment of the scientific community. In January 1981 when the minute hand of the clock, which has for years been the *Bulletin*'s cover emblem, moved from seven to four minutes to midnight, Feld's phone rang steadily for weeks with callers from all over the United States and Europe asking why, or why not further.

Feld's organizational eclecticism—not to mention his quiet perseverance—has earned him a special place among scientists who try to curb nuclear weapons. But it should not be misinterpreted, for Feld is no dilettante do-gooder. His multiple commitments to Pugwash, the Council for a Livable World, and the *Bulletin* spring from his conviction that every possible route to nuclear containment must be explored. If the world survives to read a history of the anti-nuclear weapons movement, Bernard Feld will be among its heroes.

Yet another Los Alamos alumnus who has persistently addressed the nuclear weapons problem is Victor F. Weisskopf, formerly head of MIT's Center for Theoretical Physics. Weisskopf too has worked through organizations and written articles, but of late years his chief contributions have been on the personal level, made possible by a happy combination of friendly accessibility and scientific eminence. Weisskopf is a true cosmopolitan. He grew up in Vienna, studied physics in Göttingen, Zürich and Copenhagen, and held a university post in the Soviet Union before coming to the United States in 1937. His years as director of the European Center for Nuclear Research in Geneva from 1960 to 1965 further broadened his contacts.

Last spring Weisskopf became involved in a surprising East-West exchange when he was asked by a Soviet Embassy official in Washington to answer four questions about the arms race for a Soviet survey of world opinion. One question: Did he approve of Brezhnev's proposal that an international committee of scientists be appointed to publicize the "vital necessity of preventing a nuclear catastrophe"? Weisskopf's reply: "No. Scientists have spoken many times already and they cannot add much more. What is necessary today are actions by the governments. Both the Soviet government and the U.S. government should reduce the number of nuclear weapons, instead of increasing them."

Weisskopf assumed that this blunt answer would close the episode, but two months later he received clippings from the weekly *Moscow News* containing his full reply in Russian, English, Spanish and French. Weisskopf believes that publication of his comments in the widely distributed *News* may signify a change of attitude toward arms cuts on the part of Soviet leaders. He is less sanguine about our own.

Unlike his former Los Alamos asso-

Victor Weisskopf

ciates, distinguished Harvard chemist George Kistiakowsky is a relative newcomer to the fight against nuclear weapons, although no stranger to the field. As he readily admits, he was a weapons man through the 1950s. Two years in Washington as President Eisenhower's science adviser saw the beginning of his disenchantment with high technology weaponry. Kistiakowsky admired Eisenhower, who thought more clearly than he spoke and thoroughly distrusted generals, but he was frightened by the bureaucrats who made policy without taking time to learn the facts.

Subsequent service as chairman of the President's Science Advisory Committee and on the advisory committee of the Arms Control and Disarmament Agency strengthened his belief in the sheer lunacy of ever increasing weapons overkill. Back in Cambridge, with every article he wrote and every speech he made, Kistiakowsky became a more outspoken critic of official policy, his criticism sharpened by a gift for forceful language and caustic wit.

But only within the past decade has Kistiakowsky become a visible organization man by accepting the chairmanship of the Council for a Livable World. "I've become a dignified rabble-rouser," Kistiakowsky told me recently with a characteristically wry smile. "There is no use working as an individual in Washington, at least not in the field of nuclear weapons. Scientists have either been discredited there or have lined up with the military, which is the most powerful bureaucracy in the world."

A large majority of scientists who have been involved from the beginning with nuclear energy agree with him. The issue has not caused a breach in the American scientific community; there is too much at stake. But among people opposed to weapons escalation it has been a matter of deep regret that for the past decade the scientist-initiated organization with by far the largest popular following, the Union of Concerned Scientists, has focused attention on the safety of nuclear power rather than upon the arms race.

When the Union of Concerned Scientists took formal shape in 1969 it was literally what its name implies, a group of scientists worried about the effects of science and technology on the environment. It is now the fifth largest environmental group in the country with a membership approaching 120,000.

The Union's strategy, which is to use a cadre of experts to influence policy-making directly and also through an aroused public, closely approximates that employed by the Federation of American Scientists in its early days. For that reason, and because of my interest in the nuclear power/nuclear arms issue, I talked with Henry Kendall, MIT professor of physics, who for some years has been the Union's best-known spokesman.

"The Union didn't set out to build a grass roots movement and hasn't stormed any gates," Kendall explained. The emphasis on nuclear power stemmed from the environmental concerns of the initial *ad hoc* group, and the organization grew in

Henry Kendall

response to the technical analyses that he and his colleagues provided, acting collectively as unofficial science adviser to the American people. Their findings about the safety of nuclear reactors and power plants have never been found in error, he told me with understandable pride.

But Kendall has had no quarrel with his colleagues' concern over nuclear weapons: "The nuclear arms race," he said, "is the most outstanding folly on which mankind has so far embarked —a much more important problem than nuclear power. Three Mile Island provided a useful lesson, but an arms control analogue in the form of a small war is simply not acceptable."

Will the nuclear freeze petition, the Physicians for Social Responsibility's seminars on the medical effects of nuclear weapons and the campus teach-ins produce an effective grass roots movement? What do older scientists think about this? "I see a genuine revival of anti-war consciousness in this country," Weisskopf told me, "but whether it will be strong enough to counter the increasing aggressiveness of our own government is another matter." Kistiakowsky holds much the same view of both public and official reactions. As evidence of "a strong awakening" he cites the stand taken on MX missiles by the powerful Mormon leadership which, even if not disinterested, has publicized the issue. And there is a chance that the Physicians' campaign will succeed because people are used to seeking advice from doctors. Feld, too, is encouraged by the various groups of professionals who are organizing to oppose nuclear war.

Is there a chance of avoiding nuclear war? No one dares express optimism, but Philip Morrison came close to qualifying his pessimism: "Our society is always changing," he said, "and who knows what input will be amplified? Perhaps some catalyst will do for arms control what Rachel Car-

No defense, no secret, no monopoly, therefore international control became the rallying cry.

son's *Silent Spring* did for the environmental movement." There is general agreement that ever larger stocks of increasingly complicated weapons are more likely to precipitate than to prevent war. Therefore any tactic that will reduce these arsenals is worth pursuing, including step-by-step solutions.

There is a general disposition in the arms control camp to regard supporters of a strong nuclear weapons establishment as paranoid. Morrison pointed out that a few prominent scientists here and abroad have gone so far as to say that nuclear war is inevitable because political leaders are mad. Morrison does not agree. "Indeed," he says, "elected officials are under very heavy inhibitions in regard to nuclear weapons and have no intention of using them. The danger lies in the fact that even rational leaders sometimes err."

Representatives of the new leadership expressed their own points of view. Psychiatrist Eric Chivian (International Physicians for the Prevention of Nuclear War) sees a lot of denial in public attitudes to nuclear weapons and therefore expects less from a mass movement than from an elitist group like the International Physicians. Randall Forsberg (Institute for Defense and Disarmament) thinks that it is a mistake to concentrate on nuclear arms: "As long as the liberal establishment remains open to the use of conventional weapons," she says, "there is not much hope of a secure peace." Yet, like her elders, she has adopted a pragmatic approach promoting the nuclear freeze idea on the slim chance that it may take off and point the way to other levels of agreement.

Struck by the parallel between the Physicians for Social Responsibility campaign and that of scientists after World War II, who left their laboratories for speakers' platforms across the country, I asked Helen Caldicott of Physicians for Social Responsibility whether doctors will be willing to exploit their special, very personal relationship with patients and turn propagandists for what many people will see as a political cause. Caldicott thinks they will because prevention of nuclear war so clearly falls in the category of preventive medicine.

Helen Caldicott

When I asked my older scientist friends why they think the intense activity of the immediate postwar period failed to halt escalation, Morrison's reply was the most comprehensive:

"In its own terms the movement was quite successful. It alerted an influential segment of the American public to the dangers of nuclear weapons and helped put civilians in charge of domestic programs. But the legislation failed to allow for the introduction of civilians with different views about nuclear defense from those of the people who framed it. As for international control, we vastly underestimated the force of nationalism in the postwar world."

Were there occasions in the past 30 years when more pressure or a different tactic might have affected the course of nuclear weapons development? I found general agreement that public pressure did swing the balance in favor of the Partial Nuclear Test Ban Treaty of 1963 and that this treaty is well worth having even though it does not eliminate underground testing. Feld speculates that had the Council for a Livable World provided campaign funds a year or two earlier there might have been enough support in the Senate for a comprehensive test ban. Kistiakowsky claims that the agitation in the late 1960s which led to limitations on deployment of anti-ballistic missiles came close to being a mass movement.

My own view is that the failure to capitalize more fully on the anti-war sentiment of the 1960s lies partly within the scientists' movement itself. With some notable exceptions new leadership did not develop among younger scientists in the 1950s. And in the absence of significant progress toward arms control the *Bulletin*, the Pugwash conferences and the Federation of American Scientists devoted more attention to general causes and symptoms of international tension —disparity between Third World and developed countries, threats to the environment and violations of human rights—than to arms escalation and proliferation. In the mid-1970s the emphasis began to shift back to ways and means of focusing public attention on the arms race.

In 1945, the popular image of the singleminded scientist, dividing his time between laboratory and classroom, was the model for the young science student. But this stereotype did not long survive World War II.

It is true that the decline in membership in the Federation of American Scientists after 1947 was caused in part by a feeling among some young scientists that they had better devote themselves to the laboratory. But others continued their extracurricular activities and in so doing followed the example of many of their elders who

Few people who worked on the bomb have since accepted a job or signed a contract without thinking of its broader implications.

adopted such causes as world government or joined the federal advisory apparatus. A look at the careers of outstanding Manhattan Project alumni, whose lives were probably most affected by the bomb, reveals that very few have not broadened their range of interests. They have become presidents and deans of major universities, directors of prominent research centers, or advisers to presidents—occupations fully as disruptive to the free flow of creative thought as any of the arms control activities. It would be hard to prove that X, Y or Z would have made a greater contribution to physics or chemistry had he not tried to stop the arms race.

A final question, not mine but one that I often hear: Do scientists feel guilty because they developed the bomb? To tell the truth, I never ask them. I remember too well how events of the 1930s gradually eroded my own pacifism so that in early 1943 I willingly concurred in my husband's decision to accept a job about which he could say only that it would end the war and promise thereafter enormous peacetime benefits. In any case, guilt has so many connotations, and the capacity to feel it varies so greatly, that one human being cannot rightly speak for another.

But one can speak with some assurance about responsibility—a more meaningful term in the present context, and so widespread a reaction that I venture to say few people who worked on the bomb have since accepted a job or signed a contract without thinking about its broader implications. Their decisions have reflected different conclusions as to the course that responsibility dictates. Still, the process represents a new kind of evaluation now common throughout the scientific community.

What has also long been evident is regret among scientists over the way the first atomic bombs were used—that Nagasaki followed so close on Hiroshima and, as more was learned about the strength of the Japanese peace party in August 1945, that the bombs were used at all without specific warning. But the reactions of scientists have also been influenced by their normal way of working. If an experiment fails or a theory collapses, they do not ordinarily waste time in self-reproach; they design new experiments or develop different hypotheses. That is why so many of those who helped make the bomb have not cried *mea culpa* but have instead devoted themselves unstintingly to the search for constructive solutions to the problems it created. □

KOSTA TSIPIS

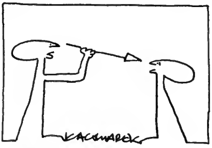
An introduction

Many people nowadays worry and wonder about the arms race. They worry because the process accumulates useless but increasingly unstable nuclear weapons systems in the arsenals of the United States and the Soviet Union. They wonder because there seems to be no military benefit in this accumulation and no logical connection between further augmentation of nuclear arsenals and the achievement of any political aims or goals: Nuclear weapons cannot resolve conflict, let alone favor one side, because of their pan-catastrophic character.

There are theories that attempt to explain the nuclear arms race. One says that it is a survival response to the danger of extinction posed by nuclear war: Since we cannot use nuclear weapons in fighting we use them to posture and try to resolve the conflict between the United States and the Soviet Union by coercive or intimidating displays of even more numerous and sophisticated weapons. Another says that the arms race in fact has nothing to do with the actual security needs of the country but is a response to the problem posed by the very existence of nuclear weapons: We have the damn things, we can't think of anything useful to do with them; but at the same time we are afraid not to do something. And since we really don't know what to do we indulge in the simplest thing—accumulating increasingly elaborate versions of them.

Herbert York's and Fred Kaplan's articles taken together suggest still another explanation—that the arms race is the by-product of operations of our armed services, acting as competing corporate entities that produce and consume their own products; that their concern is not only the Soviet threat but also the invention, promotion and assumption of strategic missions that justify the acquisition of an endless chain of new weapons systems, acquisition vital to the bureaucratic health and growth of any one service, the Air Force, for example.

York's realization of the constancy of the size of the U.S. arsenal and of Soviet efforts not to be outdone bespeaks the decoupling of nuclear weapons from defense or even political imperatives, and raises the further question: Are the quantity and quality of our nuclear arsenal relevant to our national security? It is, however, York's documented claim that the "rhetoric" of Soviet superiority has remained invariant for 35 years. This, along with Kaplan's explications, suggests the existence of a bureaucratic component to the phenomenon of the arms race: For a service like the Air Force to grow organizationally, to attract bright ambitious people and provide interesting jobs for them, to acquire power and influence in order to keep growing, it needs money. Congress, the provider of money, must be induced first to allocate enough, and second to allocate it to the Air Force rather than to a rival organization like the Navy.



"SEE MY POINT?"

The "constant rhetoric" that York discerns has remained so because it has successfully persuaded Congress and the public to accept increasingly expensive nuclear weapons systems. There is no reason to change something that has worked so well. But the pragmatic reality about nuclear weapons alters under the influence of technology and of circumstances largely beyond the control of the U.S. Armed Services—like the Soviets' buildup of their own nuclear arsenal. As Kaplan points out, such changes are, at the bureaucratic level, both opportunities and challenges to the organizational welfare of the services.

As with large corporate entities, the services welcome new arenas in which to do business, and they worry when opportunities for expansion dry up. So the "strategic thinkers" and the "persuasive briefers" invent scenarios for *using* nuclear weapons that are compatible with the changes, but which assure a continuing need to accumulate additional nuclear weapons and so keep the services "in business." Hence the arms race and its irrelevance to any actual national defense needs. The problem with this theory of the arms race is that it cannot be tested and so can never be proven right or wrong. □

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Vertical proliferation

Although the history of the U.S.-Soviet arms race has been characterized by extreme changes and fluctuations, three remarkably constant features also emerge. They are:

- the rhetoric of the U.S.-Soviet relationship, which has scarcely changed in 35 years;
- the number of strategic nuclear delivery vehicles in the U.S. arsenal, which has remained essentially the same since the Korean War; and
- the expenditure level of the Soviet Union on strategic armaments, which has been about the same fraction of their gross national product since 1964.

The rhetoric of the arms race. In 1950, a document issued by the U.S. National Security Council (NSC Report 68) describes at length the U.S.-Soviet situation as then perceived, and concludes in a very pessimistic tone about Soviet capabilities and intentions:

"The Soviet Union is developing the military capacity to support its design for world domination. The Soviet Union actually possesses forces far in excess of those necessary to defend its territory. Should a major war occur in 1950, the Soviet Union and its satellites are considered by the Joint Chiefs of Staff to be in a sufficiently advanced state of preparedness immediately to undertake and carry out campaigns to overrun western Europe, to launch air attacks against the British Isles, and to attack selected targets with atomic weapons in Alaska, Canada and the United States."

As a measure of how desperate the authors of this report felt the situation was, they concluded that a large measure of sacrifice and discipline would be demanded of the American people, who "will be asked to give up some of the benefits they have come to associate with their freedoms." This desperation concerned a situation that was expected to develop *within the next few years after 1950.*

Only seven years later, the highly publicized Gaither report concluded:

"The evidence clearly indicates an increasing threat which may become critical in 1959 or in early 1960. The evidence further suggests the urgency of the proper time phasing of needed improvements in our military position vis-à-vis Russia. The singleness of purpose with which they have pressed their military-centered industrial development has led to spectacular progress. They have developed a spectrum of A- and H-bombs and produced fission material sufficient for at least 1,500 nuclear weapons and they have probably surpassed us in ICBM development."

The Gaither report called for a large number of emergency measures for the United States, including, particularly, a national civil defense program.

By simply changing a few of the nouns in these reports, one could convert them into reports that are in wide circulation today, and that deliver essentially the same message. For example, the terms used today to present the problems of "Minutemen vulnerability" and the "civil-defense gap" are remarkably similar to those used to describe other gaps over and over again for the past 35 years. Also, for all these years, the predictions in these reports have been wrong. Of course this does not prove that similar predictions are wrong today, but it does mean that a healthy degree of skepticism is warranted regarding contemporary predictions about the future of the U.S.-Soviet situation, even when they are made by very prestigious individuals or groups.

Perhaps some skepticism is also warranted about the credibility of *people* who have made dire predictions in the past that have always proved to be incorrect, and who continue to make such predictions. If you live where there are wolves, the person who says every day that there will soon be a wolf at your door may turn out to be right some day, but this is not a person whose insights into the future would, or should, inspire your confidence. Yet some of the same people who have been saying such things in the United States, and who authored reports such as those quoted, are still in positions of considerable influence with respect to American defense policy.

The arsenal of strategic weapons. The strategic nuclear arsenals of the United States and the Soviet Union are usually described in terms of type of vehicle, type and size of warhead (megatons), number of delivery vehicles of each type, vehicle speed, accuracy, details of construction and so on. Of these factors, the number of strategic delivery vehicles is the one that receives by far the most attention in U.S. Congressional budget hear-

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ings and other internal debates, as well as in the Strategic Arms Limitation Talks (SALT) between the two countries.

Shortly after the Korean War, one of the nuclear policies then being developed by the new Eisenhower Administration was the policy of "massive retaliation," which implied a full-scale U.S. nuclear response in the event of a serious Soviet expansionist move. At that time, the actual implementation of the U.S. strategic arsenal jelled in such a way that the number of strategic delivery vehicles came out to be just under 2,000. Today, a quarter of a century later, the number of delivery vehicles is 2,200 and, in fact, since 1955 this number has not changed by more than 5 percent on the average, with a maximum deviation of only 9 percent. The latter occurred when the number went up to 2,400 for a period of about one year following the Cuban missile crisis. Thus, over a 30-year period during which almost everything else that relates to the arms race changed wildly, the number of U.S. strategic nuclear delivery vehicles remained essentially constant.

Although it is not easy to understand why this number has remained so steady, the history of how it came about is more straightforward. The number was determined not from strategic nuclear thinking, but as the result of an internal debate and compromise between the bomber generals from World War II and the government's budget directors. The bomber generals, who had planned and carried out the air war against Germany and Japan, thought in conventional World War II terms of the large numbers of bomber aircraft required for penetration in sufficient force to overwhelm defenses. They were applying this traditional experience to the utterly different and unprecedented situation of nuclear weapons, and were thinking in terms of large numbers of wings, squadrons and aircraft. The budget directors, however, were thinking in terms of holding costs down. The two groups compromised at a number in the neighborhood of 2,000.

Since that time almost every other feature of the nuclear arms race changed dramatically. The first-generation atomic bombs were replaced with hydrogen bombs, with 100 to 1,000 times more destructive power, yet there was never any discussion about decreasing the number of bombs. The total destructive power of the U.S. arsenal thereafter increased greatly, reaching a maximum in about 1960. But it has been decreasing ever since, because of another factor that was changing rapidly over this period.

At the beginning, the delivery vehicles were mostly B-29 and B-36 propeller-driven aircraft of World War II vintage, with a small number of B-47 first-generation jet bombers. As time went on, jet aircraft, especially the B-52, became a larger and eventually predominant proportion of the bomber fleet. By 1960, ballistic missiles were being deployed: Thor and Jupiter in Europe, Atlas in the United States and Polaris at sea. But the missiles then had much less payload-carrying capability than aircraft, and they could carry only one warhead each. The result of this evolution of bombers to missiles was, therefore, that the number of available megatons of destructive power decreased considerably. In fact, it never returned to the earlier level of the manned bomber period, and today U.S. forces have about one-third the megatonnage that they had in 1960.

The number of warheads changed in a different way, first dropping rapidly as the missiles were deployed in the 1960s, then increasing again as multiple-independently targeted reentry vehicles (MIRV) were introduced in 1970, making possible more than one warhead on a rocket. Through all these changes, however, the number of delivery vehicles remained essentially the same; every time one new missile was introduced into the force, one old airplane was removed. Although there have been numerous suggestions that the number of delivery vehicles should be altered because of the many other changes that had occurred—including the greatly increased accuracy of recent systems such as the Cruise—this in fact never happened.

The political situation was also changing radically. When the doctrine of massive retaliation was formulated in the early 1950s, a widespread belief existed in the United States that there was a monolithic Sino-Soviet bloc bent on territorial expansion, and further, that this country would be forced to employ technological means to cope with a massive ground-force invasion of Europe. In 1960, of course, the Sino-Soviet bloc disintegrated, but even so great a political change as this did not cause a change in the number of strategic nuclear delivery vehicles. The one event that did precipitate a small change was the Cuban missile crisis: a slight increase of 9 percent in the strategic force occurred because President Kennedy decided that, at that particular time, removing B-47s from the force would send a misleading signal to the Russians, so there was a period when B-47s were not being decommissioned as rapidly as Minuteman missiles were being brought on line. Evidently this did not make much military sense, and within one year enough B-47s were decommissioned to bring the number of missiles back to 2,200.

Other important political developments were taking place: The United States and the Soviet Union entered a period of political detente; yet the missile force did not change. The Stra-

tegic Arms Limitation Talks got underway; the missile force did not change. In fact, the basis for the figures brought to the SALT discussions was the existing force, and the plan of SALT was to continue the force at this level indefinitely.

It is interesting to contemplate why the U.S. missile force should have remained essentially constant through the many important and relevant political and military changes that took place in the 1950s and 1960s. If the succession of strategic and operations analysts through that period thought that they were actually *deciding* what the force would be, they were wrong, as no series of plans that took into account all those changing circumstances would have, as if by magic, all come up with the same number—2,200. The people who thought they were planning the force were actually rationalizing it.

Another example of rationalizing concerns the way the target system for the U.S. missile force seems to be derived. One might assume that in reasonable strategic force planning, the number of strategically important targets would first be defined, and then the force would be appropriately designed relative to that number. But this is not the way it has actually worked: the number of targets has in fact become equal to the number of available re-entry vehicles. In other words, the target system is based on the force size rather than vice versa. And unfortunately, this has been the case for a long time. As far back as the late 1940s, when David Lilienthal was chairman of the Atomic Energy Commission, he complained publicly that his job was to produce weapons in the required numbers, but when he asked what the requirements were, the only response he ever got was that there should be "more."

Soviet expenditures in strategic systems. Another of the most important

It is not simply that the basic theory underlying the arms race is wrong; rather it is that there is no underlying theory at all.

facts, and constants, of the arms race is that the level of the Soviet Union's investment in strategic nuclear forces has since 1964 been an essentially constant fraction of their gross national product. Therefore, this expenditure slowly but very steadily rises, and apparently does so regardless of what else is happening in the world. We do not know how or why this came about, but it seems no more coincidental than the constancy of the U.S. missile force. Unlike the United States, however, where the expenditures have fluctuated wildly but the force has remained constant, the Soviet situation has been the reverse.

From 1964 to 1974, the Soviets built up their missile force very rapidly, from a few hundred delivery vehicles to some 2,400 by the middle 1970s. Since that time, perhaps as the result of the SALT I talks, the Soviet force has not increased in numbers. What has happened instead is that many improvements in and new models of delivery vehicles have been introduced. One result of the Soviet approach is that the number of models is very much larger than in the United States. Since 1960 the United States has introduced the Atlas, the Titan, two models of the Minuteman, and now the MX. During the same period, the Soviets have moved from the SS-5 all the way to the SS-25—essentially 20 different systems, often with a number of modifications of each. The Soviet missile-design bureaus evidently work at a constant level of effort, steadily turning out new and improved systems. The result of this mode of operation was to increase the number of missiles until the middle 1970s, but since then it has served to introduce a greater variety and also improvements into the system.

Nothing that has happened outside the Soviet Union since the Cuban missile crisis (which probably *did* have a great influence on the Soviet effort) has appeared to influence their course. Relations with China steadily worsened; it made no difference. Detente came along; it made no difference. SALT came along; it made no difference. Although the SALT negotiations had some influence on Soviet missile deployment, they did not influence the level of investment in their total strategic program. Now U.S.-Soviet relations have again changed for the worse, since the Soviet involvement in Afghanistan, but there are again no signs of any change in the pattern of Soviet investment in strategic systems.

The Soviet effort has clearly borne fruit. They have produced a capable development system and good, high quality equipment. They have reached approximate parity with the United States in the various factors that are important in the nuclear arms race and they may very well surpass us.

Is there a way out? When we reflect on these three constants of the nuclear arms race, we can only conclude that the arms race really does have a "mad momentum of its own," as former Secretary of Defense Robert McNamara once remarked; that it is as mindless and as dangerous as its most radical opponents say it is. It is not simply that the basic theory underlying the arms race is wrong; rather it is that there is really no underlying theory at all.

Ultimately, the solution to the arms race must be found in the political arena, because it arose out of problems that are basically political. This will have to come about through a very profound evolution of the present nation-state system, which currently is characterized by 160 independent actors with almost no body of law—and absolutely no law enforcement—governing the relations among them. Before that millennium arrives, however, we are obliged to pursue lesser possibilities. One of the most important is direct negotiation with the Soviets and others, designed first to limit the arms race, and then to reverse it. Although we have been attempting this course for some 35 years and do not have much to show for it, the present situation would probably be even worse if we had not been making this effort.

In addition, there are certain limited unilateral actions that are perfectly sound, in the sense that they would not reduce national security, and that would move the world in the right direction. One example would be a pledge of "no first use" of nuclear weapons. Another would be the elimination of battlefield nuclear weapons, which are designed to be used in actual warfare and have deterrence only as a secondary purpose. It would of course be preferable if both of these steps could be negotiated bilaterally, but even taken unilaterally they would be important steps forward. □

Strategic thinkers

Among those who ponder the size of nuclear arsenals and the wisdom of nuclear arms control, the question most commonly debated is "How much is enough?" Yet too often neglected is the more pertinent question: "Enough for what?"

Those who point only to the superpowers' nuclear "overkill" believe that one side has "enough" nuclear weapons when it can demolish the other side's major cities in a retaliatory second-strike—a prospect so horrifying that it will deter an enemy from launching a nuclear first strike even under desperate circumstances. Yet American strategic thought—and strategic policy—has long been governed by the premise that these weapons might one day be used and, if they are, that the United States should somehow emerge victorious, or at least alive and breathing. Thus, how to *fight*—and, some would add, win—a nuclear war has always been the first question for many officials. And that approach presents an entirely different formula for calculating how much is enough.

Those who believe that deterrence should be the sole policy and that this requires only a small nuclear arsenal generally take their cues from the earliest writings of Bernard Brodie, the father of nuclear strategy, who in 1946, just months after Hiroshima, wrote:

"The first and most vital step in any American security program for the age of atomic bombs is to take measures to guarantee to ourselves in case of attack the possibility of retaliation in kind. The writer . . . is not for the moment concerned about who will *win* the next war in which atomic bombs are used. Thus far the chief purpose of our military establishment has been to win wars. From now on its chief purpose must be to avert them. It can have almost no other useful purpose."[1]

Brodie also said:

"The number of critical targets is quite limited. . . . That does not mean that additional hits would be useless but simply that diminishing returns would set in early; and after the cities of, say, 100,000 population were eliminated, the returns from additional bombs would decline drastically. . . . If 2000 bombs in the hands of either party is enough to destroy entirely the economy of the other, the fact that one side has 6000 and the other 2000 will be of relatively small significance."[2]

Yet only a few years later, Brodie himself, in a development not widely acknowledged or known by many of his celebrators, started to adopt a different standard for assessing atomic adequacy.

As the 1950s began, the Soviets had virtually no atomic bombs and the United States had a few hundred. The fear among the American military was not that the Soviets would attack the United States but that they would invade Western Europe, economically prostrate after World War II, physically unable to defend itself. Since the United States had no conventional forces to speak of, and certainly none that could quickly be mobilized to Western Europe, the only hope for defense and deterrence was the atomic bomb.

Brodie was briefly consultant to Air Force Chief of Staff General Hoyt Vandenberg at the beginning of the decade, and was assigned the task of examining the atomic war plan.[3] Brodie discovered that virtually no one had figured out precisely how the bomb was to push back or wipe out the Soviet military. The U.S. Strategic Air Command (SAC), according to the war plan, was simply to drop all of its bombs as quickly as possible on targets inside the Soviet Union, destroying as many key factories and—as long as they were close to other targets—military facilities as it could in a single volley. The Air Force called the strategy "the Sunday Punch," and saw it as the optimal way of "killing a nation."[4]

Brodie had doubts. For one thing, the Soviets were in the process of acquiring their own atomic arsenal. If the United States responded to Soviet conventional aggression by "killing" the Soviet Union, the Kremlin would almost certainly retaliate in kind. Thus, to execute SAC's war plan would be tantamount to committing suicide. The nineteenth-century warrior-philosopher, Karl von Clausewitz, had said that "war is the continuation of politics by other means." Brodie, an admirer of Clausewitz, interpreted that to mean that wars must be fought to accomplish rational objectives and that the degree of destruction should be proportional to the value of the objective. National suicide, of course, was hardly a rational objective, what-

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ever the value of the stakes. The SAC plan, therefore, was not only a fatuous war strategy; it might not even serve well as a deterrent, since the Soviets might suspect that no sane U.S. President would carry out such a self-defeating threat.[5]

Brodie conceived an alternative plan: If the Soviets, say, invaded Western Europe, the United States should fire only a few of its nuclear weapons—perhaps against Soviet troops on the battlefield, certainly *not* against Soviet cities. The United States should also have deployed a highly secure, essentially invulnerable reserve force of nuclear weapons. After this limited strike, the United States should insist that the Soviets stop their aggression at once and, threaten to use the reserve force against Soviet cities, if they did not stop. The hope was that this carrot-and-stick combination would effect a Soviet surrender or, short of that, keep the battle limited to the battlefield.[6]

In the early 1950s, neither the Air Force nor the rest of government paid much attention to Brodie's idea. Indeed, a few years later, President Eisenhower and Secretary of State John Foster Dulles proclaimed a policy of "massive retaliation" very similar to the SAC war plan.[7]

But Brodie did influence others. In 1951 he joined the RAND Corporation, an Air Force-sponsored "think tank" specializing in military research. There several other budding strategists were attracted by his ideas. One was Herman Kahn, who later explored nuclear-warfighting scenarios in three influential books: *On Thermonuclear War*, *On Escalation* and *Thinking about the Unthinkable.*[8] Another was Andrew Marshall, since 1974 director of the Pentagon's net assessment office. Marshall later had a major role in drafting the controversial "Defense Guidance" by Secretary of Defense Caspar Weinberger, which calls for the ability to fight a "protracted" nuclear war.[9]

Brodie had dealt more with what targets should *not* be hit with nuclear weapons than with those that should. An answer to the latter question presented itself—not so much to Brodie as to his colleagues—in 1953, when the Soviet Union detonated its first hydrogen bomb. The United States had exploded one a year earlier, with several times the power of the first Soviet effort. But the Soviet test indicated that they were much farther along than the United States in developing a hydrogen bomb that could be attached to an intercontinental ballistic missile. (ICBM).[10] Suddenly, America was entering a new state of vulnerability. And to some of the RAND strategists the appropriate target for Brodie's limited-strike strategy was clear: the Soviet strategic nuclear arsenal. Sparing Soviet cities in a nuclear attack might *induce* the Soviets to avoid American cities; knocking out their long-range weapons would *prevent* them from doing so.

Thus, the tentative beginnings of what would later be known as a "counterforce/no-cities strategy" began to penetrate the strategic community at RAND. More systematic thinking about the issue was inhibited, however, by the concern that the United States would never be able to find all the Soviet military targets, making such a strategy unworkable.

By the end of the 1950s this obstacle collapsed. The U-2 spy plane and the impending development of the Discoverer reconnaissance satellite —which a few at RAND, including Marshall, knew about—made counterforce seem feasible. The theory was taken up again, most actively by a RAND historian and political scientist named William W. Kaufmann. Like his former teacher Bernard Brodie, Kaufmann had done a great deal of writing on limited (conventional) warfare in the mid-1950s.[11]

For a few bright, ambitious officers in the Air Force staff, Kaufmann and counterforce came along at just the right time. The Air Force was facing, from the U.S. Navy, a fearsome threat, far more severe than any the Soviets had ever posed. This was the Navy's new Polaris submarine. Unlike SAC's bombers, which sat on airfields that were increasingly seen as vulnerable to attack, Polaris moved underwater, undetected. Its submarine-launched missiles (SLBMs) travelled at hypersonic speeds in a ballistic trajectory and were therefore invulnerable to Soviet air defenses as well. And Polaris could destroy Soviet cities and many other targets just as easily as the bombers could. Since SAC's war plan called for hitting urban and military targets simultaneously—with an emphasis on destroying industrial plants in enemy cities—SAC seemed dangerously on the edge of obsolescence.

A new strategy was needed at once, and RAND's counterforce/no-cities idea seemed just the thing.[12] By late 1960, counterforce/no-cities became the official Air Force policy.[13] The Air Force liked counterforce because one weakness of Polaris was that its missiles were dreadfully inaccurate; they could not be relied upon to strike military targets without hitting nearby cities as well. It was questionable whether SAC's bombers would be perfect at the task, either, but they would have a much better chance.

The Navy had its own "new strategy," designed to rationalize Polaris and further discredit SAC. Called "finite deterrence," its tenets were explained in a widely circulated but classified document called Naval Warfare Analysis Group (NAVWAG) Study No. 5, "National Policy Implications of Atomic Parity."

The idea was in essence a refine-

As long as both sides kept building weapons . . . there would be more and more targets, requiring more and more weapons. . . .

ment of the strategy that Brodie had outlined 15 years earlier in *The Absolute Weapon*: Deterring an enemy nuclear attack required having enough nuclear weapons so that in the event of an enemy first strike the United States could obliterate the urban-industrial society of the aggressor with the weapons that survived. The most efficient way to accomplish this, according to the NAVWAG, was to have a relatively small number of nuclear-armed submarines stationed at sea at all times.

That was all we needed; no matter how many weapons the Soviets built, the United States would be in fine shape as long as the submarines were there. Building more bombers or land-based missiles would only guarantee an unending arms race.[14]

Against this last point the Air Force had no argument. An arms race was exactly what they wanted—as long as they were given the money and weapons to win it. For them, the beauty of counterforce, was that—unlike finite deterrence—it prescribed no logical limit to the number of weapons that were "needed." As long as both sides kept building weapons—as they almost certainly would—there would be more and more targets, requiring more and more weapons with which to destroy them.

By the time John F. Kennedy took office in January 1961, the issues of nuclear strategy had become thoroughly intertwined with a major interservice rivalry over which branch of the military would receive the bigger budget. This Air Force-Navy competition would dominate the U.S. side of the nuclear arms race and the strategic debate through the 1960s.

When Robert McNamara became Kennedy's Secretary of Defense, he initially liked the idea of "finite deterrence": It provided a clear measure of how much was enough, and systematic guidance on how tightly he could rein in the military, over which he wanted to establish complete control.[15] But McNamara had also hired some RAND strategists as his top assistants—most notably Charles Hitch as comptroller and Alain Enthoven as Hitch's deputy for systems analysis. Advocates of the counterforce philosophy, these two were distressed by McNamara's penchant for finite deterrence. They persuaded their boss to give William Kaufmann's views a hearing and set up an appointment for February 10, 1961.

A week earlier, McNamara had visited SAC headquarters in Omaha for a briefing on the Single Integrated Operational Plan (SIOP), the military's nuclear war plan. Basically, it was only a slight variation on the old SAC plan—kill and destroy as much as possible in the Soviet Union, Red China and Eastern Europe, as quickly as possible; there were virtually no provisions for more limited options. McNamara was appalled by the massive devastation called for, by the straitjacket in which it would strap a President in a crisis. He was looking for some way to give the President more options—and Kaufmann's briefing showed him how.

McNamara adopted the counterforce/no-cities strategy in his first few years as Defense Secretary, espousing its philosophy at Athens in a top secret speech to the NATO Defense Ministers in May 1962 and in a public commencement address at the University of Michigan the following June.[16]

At the same time, however, McNamara was cutting Air Force programs by the handful—the B-70 bomber, various primitive cruise missiles, the Skybolt air-launched ballistic missile—and limiting the number of B-52 bombers and Minuteman ICBMs to be deployed.[17] The Air Force had its counterforce strategy, but not the budget or the weapons for which it had adopted counterforce as a rationale. Top officers started to apply ferocious pressure on McNamara, all the while using his own endorsement of counterforce to support their demands for more money.

The Secretary fought back by dispensing with the counterforce rhetoric. He and his systems analysts devised a new measure of nuclear adequacy called "assured destruction" which, over the years became known as "mutual assured destruction" or MAD. It stated that the United States had enough nuclear weapons when, following a Soviet first strike, it could still kill one fourth of the Soviet population and half its industry. This task required—so went their calculations—the equivalent of 400 one-megaton bombs. McNamara extended this to note that there should be 400 "equivalent megatons" on each leg of the strategic Triad—the intercontinental ballistic missiles (ICBMs), submarine-launched ballistic missiles (SLBMs) and bombers—so that if two legs failed or were destroyed, there would still be one that could wreak the required devastation. Assured destruction thus would be met by 1,200 equivalent megatons surviving a Soviet attack.[18]

McNamara's aides worked out this calculation most formally in 1964. As it happened, his five-year defense plan for that year—which recommended several hundred fewer nuclear warheads than the military wanted—provided enough weapons so that by 1969, after a fairly successful Soviet first strike, the United States could respond with exactly 1,200 equivalent megatons.[19] The assured-destruction philosophy was, in essence, a political technique that appeared to give scientific justification to McNamara's own weapons plan in the face of Air Force opposition.

At the same time, McNamara was having genuine doubts about counterforce. A top secret study (since declas-

If nuclear war does come, surely a president should have choices beyond suicide or surrender. . . .

sified), titled "Damage Limiting," was produced in 1964 by General Glenn Kent, under the auspices of Harold Brown's Directorate of Defense Research and Engineering. It suggested that even with a very good U.S. counterforce strike—supplemented by air, anti-ballistic-missile (ABM) and civil defense—the Soviets, in a retaliatory strike, could still inflict tremendous damage. Moreover, if the United States spent a great deal more on these defensive efforts, the Soviets could nullify the additional expenditure much more cheaply by adding only slightly to their strategic offensive forces.[20] Counterforce, in short, appeared to be a loser's game.

In addition, it appeared from their own writings that the Soviets did not believe in using nuclear weapons as tit-for-tat instruments, or in carefully differentiating urban from military targets. It took two to play the counterforce game, and if the Soviets refused to go along, it made no sense at all.

Nevertheless, even as McNamara's disenchanment grew and as assured destruction became the *declaratory* strategy, the *actual* strategy—as reflected in the targeting plan at SAC—remained predominantly counterforce. And new weapons were built to accommodate it. As the Soviets built more and more ICBMs in the 1960s, the United States responded by building MIRVs (multiple independently targetable re-entry vehicles) that allowed a single missile to strike several different targets hundreds of miles from one another.[21] As the Soviets began to follow America's prudent example of encasing their ICBMs in hardened silos, the United States built new inertial guidance systems that made its missiles—theoretically, anyway—more accurate.

MIRVs and improved guidance systems were approved by McNamara, mainly as part of a political trade-off in which the Air Force agreed to hold the line at 1,000 Minuteman missiles. It was, in retrospect, a shortsighted deal, for the two programs kept the flame of counterforce burning.

By the 1970s, a mismatch cropped up in U.S. strategic forces. The Soviets had built still more ICBMs and hardened their silos even further. The United States had continued to MIRV hundreds of Minuteman missiles and to improve the guidance systems. But it still wasn't enough: 1,650 warheads on 550 Minuteman III missiles could not destroy all 1,400 Soviet ICBM silos. (It is generally estimated that at least two warheads are needed to destroy a single hardened missile silo, since some will fall outside the "lethal radius" and some others will not work at all.)

So the Air Force, in 1973, asked for a new ICBM—the MX—which would have ten warheads and still better accuracy. With 200 MX missiles—or even 100 combined with the Minuteman IIIs—counterforce could be feasible once more.

Meanwhile, in the 1970s, the Navy had also equipped its submarine-launched ballistic missile force with MIRVs: Poseidon and Trident I. As a result, the Navy had more warheads than were required for assured destruction or for the NAVWAG-5's strategy. Yet not even in theory were these missiles accurate enough to destroy hardened silos. In the mid-1970s as the second Poseidon submarine went to sea—each with 16 missiles carrying up to ten warheads per missile—Admiral Gerald Miller, then deputy director of SAC's Joint Strategic Target Planning Staff, complained to an aide: "Hell, what are we going to do with all of those?" New targets had to be created for new weapons—not the other way around.

So the Navy had no objection when, in the late 1970s, Andrew Marshall, the RAND veteran who had become director of Pentagon net assessment, started lobbying for a program to improve SLBM accuracy to give it "hard-target-kill capability." The result was the Trident II missile, now in research and development, scheduled for operations in the late 1980s.

The Navy could now afford to get into the counterforce business, since the strategy was no longer a source of interservice rivalry. Even the Army grew cooperative when all agreed that the anti-ballistic missile, the Army's only piece of the strategic-nuclear pie, might play a part in helping to protect the MX from nuclear attack. The entire military establishment could comfortably close ranks around a common stake in counterforce. The link between strategy, force levels and higher budgets suited all.

However, several theorists—and a small number of military officers—were beginning to detect further problems with counterforce. The Soviet Union had built so many ICBMs (1,000 by 1967) that a full-scale U.S. counterforce strike would require firing at least 2,000 nuclear warheads. The problem was that the Soviets would probably be unable to distinguish such a massive strike from an all-out attack against their cities and would probably order full-scale retaliation. Thus, by the late 1960s, the requirements of a successful counterforce mission had become inconsistent with the philosophy underlying that strategy.

Thomas Schelling, a strategic theorist from RAND and Harvard, recognized this potential contradiction as early as 1960. His solution: If nuclear weapons are to be used, fire small-scale, shot-across-the-bow strikes.[22] The idea was the same as that outlined by Brodie in 1951: Inflict pain, threaten more pain as a way of coercing the Soviets to stop their aggression, but do so in a way that avoids striking Soviet cities and thus compels the Soviets to keep the conflict limited.

The idea appealed to another RAND theorist, James Schlesinger, who elaborated on it in a series of RAND studies, done in association with the Air Force in the late 1960s, known as NUOPTS, or Nuclear Operations. By the time Schlesinger became Secretary of Defense in 1973, several officials in the national-security bureaucracy had become interested, and Schlesinger was determined to make it policy.

The result was a series of National Security Decision Memoranda, beginning with NSDM-242 in January 1974 and climaxing in 1980 with President Carter's controversial Presidential Directive 59.[23] The 1982 "Defense Guidance" signed by Weinberger is only a slight elaboration of the strategy of counterforce and small-scale strikes first articulated in the 1950s.

Force requirements for this sort of nuclear strategy are less elaborate than for the full-scale counterforce plan, in that a military adopting such a strategy would not need so many weapons since it would not have to hit all of the adversary's counterforce targets. But they are more elaborate in that the weapons would have to be even more precise, and because the command-control-communications network would have to be almost unimaginably extensive and durable.

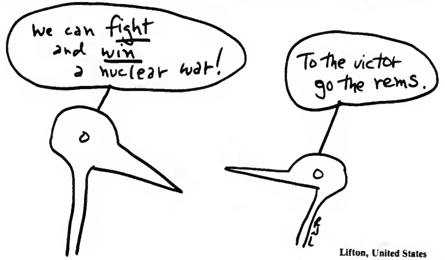
Both sides would need intelligence facilities—satellites and sensors—that could immediately inform their top officials of what targets were hit and what weapons remained. The links connecting the officials and the weapons would have to remain under firm centralized control for days, weeks or longer. This is practically impossible, given the inevitable confusion of large-scale organizations, the inherent unreliability of much military electronic gear and the vulnerability of this equipment to a wide array of nuclear effects, including blast, radiation and electromagnetic pulse. Indeed, these systems are so *inherently* vulnerable that after one or two nuclear "exchanges," chances are very high that escalation will slip completely out of anyone's control.[24]

The strategy also shares certain problems with those of the counterforce philosophy: How do you get the Soviets to play according to the same rules? Does this combination of strikes and threats really coerce the other side? Couldn't the other side fire back equally small-scale strikes—or simply pretend to ignore our "signals"—and try to coerce us instead? Finally, how do you end such a war, on what terms, on what basis of shared trust? After the initial volley, strategy vanishes; only luck and prayer remain.

Some theorists have argued that Soviet surrender would be facilitated if the U.S. targeting plan were geared to destroy the political infrastructure of the Soviet Union; that we should "take out" command posts and other key facilities of the party and military apparatus; that then the Soviet Union would collapse.[25] The real-life technicians of nuclear targeting—the Joint Strategic Planning Staff in Omaha—have considered these suggestions but have discovered no way to make them work. Furthermore by destroying the enemy's command-control network, we might be eliminating the only means by which the Kremlim could communicate with us and signal a desire to end the fighting. We would also be destroying the means by which the top Soviet political leaders could keep their own military officers under control. Authority to use nuclear weapons might automatically fall to some "mad marshal" or "crazy colonel," making further escalation and mutual destruction more likely.

Finally, there are those, mainly in Army circles, who talk of fighting a nuclear war on the battlefield. The idea goes back to the early 1950s, when a number of scientists and theorists—Bernard Brodie among them but more notably J. Robert Oppenheimer and later Henry Kissinger—wanted to avoid the holocaust of the hydrogen bomb and city-bombing by "bringing the battle back to the battlefield."[26]

But after 30 years of thinking, nobody has devised a way of doing this. Towns in West Germany, where a theater nuclear war would probably be fought, are only two kilotons apart, so to speak. If tactical nuclear weapons were used on a militarily meaningful scale millions of West Europeans would die—and that assumes no Soviet retaliation.[27] Moreover, pressure would logically build to "take out" Soviet SS-4, SS-5 and SS-20 missiles, the weapons that



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The 1982 Defense Guidance is only a slight elaboration of the strategy of counterforce and small-scale strikes first articulated in the 1950s.

most lethally threaten Western Europe. Those missiles lie inside the Soviet Union; once Soviet territory is hit, American territory would almost certainly be next. Then the problems of the counterforce and small-strike strategies start all over again.[28]

In one sense, it is understandable that some analysts have sought in earnest for the rational war-fighting option. No one, after all, really knows what deters war, not even nuclear war; and if nuclear war does come, surely a president should have—and, in fact, does have—choices beyond "suicide or surrender." The problem, however, is that there simply does not appear to be any way that one can fight, much less win, a nuclear war. For an arms controller, that conclusion has inspiring potential. The vast bulk of both sides' nuclear arsenals—practically the entire land-based-ICBM force, for example—need not exist except for the purpose of war-fighting. Therefore, if both sides ever begin to comprehend the futility of the war-fighting mission, the prospects for serious mutual arms reductions should shine with hope.

However, the bright lights dim once again with a quick look at the political realities that underlie the arms race. The military and its allies—not only in the United States, but doubtless in the Soviet Union as well—have an immense stake in the war-fighting mission and the weapons that become "requirements" as a result. Since these interests play a powerful role in both sides' politics, especially their arms control politics, the vision of massive reductions will—short of a drastic change in these politics—certainly not be realized for a very long time. ☐

1. B. Brodie, *The Absolute Weapon* (New York: Harcourt Brace, 1946), p. 76.

2. B. Brodie, p. 48.

3. For Brodie's Air Force experiences, communication to David A. Rosenberg, August 22, 1977 in *Bernard Brodie Papers*, Box 9, Pending folder, UCLA. Brodie's switch to thinking about fighting, not just deterring, war actually occurred a few years earlier. See also Brodie, "The Atom Bomb As Policy Maker," *Foreign Affairs* (Oct. 1948); "New Techniques of War and National Policies," in W.F. Ogburn, ed., *Technology and International Relations* (University of Chicago Press, 1949). Later in life he reverted to an anti-warfighting position; see Brodie, "The Development of Nuclear Strategy," *International Security* (Spring 1978).

4. Robert F. Futrell, *Ideas, Concepts, Doctrine: A History of Basic Thinking in the United States Air Force, 1907-64* (Maxwell AFB, Ala.: Air University, 1971), p. 122.

5. *Brodie Papers*, "Characteristics of a Sound Strategy," March 17, 1952; "Changing Capabilities and War Objectives," Box 12; Brodie. Also communication to Rosenberg.

6. *Brodie Papers*, Box 12.

7. John Foster Dulles, "The Evolution of Foreign Policy," *Department of State Bulletin* (Jan. 25, 1954).

8. Herman Kahn, *On Thermonuclear War* (Princeton University Press, 1960); *Thinking About the Unthinkable* (New York: Horizon Press, 1962); *On Escalation* (New York: Praeger, 1968).

9. Richard Halloran, "First Strategy for Fighting a Long Nuclear War," New York Times, May 30, 1982. That Marshall was involved comes from my own interviews.

10. The evidence was that traces of lithium were found amid the radioactive fallout from the test, indicating that the Soviet H-bomb, unlike our own, did not require a massive refrigeration unit, suggesting that it was farther along the way toward becoming a usable weapon. Roy Neal, *Ace in the Hole: The Story of the Minuteman Missile* (Garden City, N.Y.: Doubleday, 1962) and interviews.

11. W.W. Kaufmann, *Military Policy and National Security* (Princeton University Press, 1956); B. Brodie, "Unlimited Weapons and Limited War," *The Reporter* (Nov. 18, 1954).

12. *Thomas White Papers*, Library of Congress communications: General Thomas Power to General White, May 9, 1959, and General Hewitt to General Charles Westover, May 12, 1959, both in Box 27, SAC folder; White to Power, May 11, 1959, Box 29, 1959 Top Secret File Folder (recently declassified).

13. *Thomas White Papers*, communication, White to Frank Collbohm, Aug. 12, 1960, Box 37, RAND folder; "Air Force Information Policy Letter" Oct. 1, 1960, Box 37 Commanders' Conference folder; "Subjects of Major Importance for Discussion at the Commanders' Conference, 17-18 November 1960," Tab A, Nov. 1, 1960, Box 41, 1960 Top Secret File Folder (recently declassified).

14. "Unclassified Summary of NAVWAG Study No. 5," Jan. 22, 1958, White House Office files, Office of Staff Secretary, Subject Series, Alpha Subseries, Box 21, Nuclear Exchange (1) folder, Dwight Eisenhower Library.

15. McNamara got a briefing endorsing finite-deterrence position, Jan. 26, 1961, from the Weapons Systems Evaluation Group, which had just completed a major strategic study called WSEG-50.

16. The Athens speech is now declassified; for Ann Arbor speech see William W. Kaufmann, *The McNamara Strategy* (New York: Harper & Row, 1964).

17. Alain C. Enthoven and K. Wayne Smith, *How Much Is Enough* (New York: Harper & Row, 1971.)

18. Enthoven and Smith.

19. Robert McNamara, communication to Lyndon Johnson, "Recommended FY 1965-69 Strategic Retaliatory Forces," Dec. 6, 1963; "Recommended FY 1966-70 Program for Strategic Offensive Forces, Continental Air and Missile Defense Forces, and Civil Defense," Dec. 3, 1964 (recently declassified).

20. Director of Defense Research and Engineering, *Damage Limiting: A Rationale for the Allocation of Resources by the U.S. and the USSR* (Jan. 21, 1964) (recently declassified); interviews.

21. MIRVs were justified publicly as ABM penetrators, but they were always intended as weapons that could cover an expanding Soviet strategic force without having to build more missiles. See John S. Foster, quoted in Ralph Lapp, *Arms Without Doubt* (New York: Cowles Book Co., 1970), p. 21.

22. Thomas Schelling, *The Strategy of Conflict* (Oxford University Press, 1960), pp. 252-53; interviews.

23. Desmond Ball, "Counterforce Targeting: How New? How Viable?" *Arms Control Today* (Feb. 1981); Fred Kaplan, "Going Native Without a Field Map," *Columbia Journalism Review* (Jan./Feb., 1981).

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THOMAS J. DOWNEY

START . . . SALT . . . the freeze

The strategic arms reduction talks (START) are finally under way in Geneva, but is the Administration's arms reduction proposal the Holy Grail of arms control, as President Reagan would have us believe?

At best, START will take half a decade to negotiate. But we could probably have a mutual and verifiable nuclear freeze in only a year or so. And we could have the second strategic arms limitation treaty—SALT II—today, if the Administration were willing to ratify it.

President Reagan's arms reduction would probably do more good than harm, but it isn't worth the wait. A limited and narrow proposal, it reduces each side's total deployed ballistic missiles to 850, deployed intercontinental ballistic missiles to 425 and warheads to about six per missile. That's all it does.

The freeze and SALT II, by comparison, are far-reaching solutions to a wide variety of military and arms control problems. The freeze would, immediately upon ratification, prohibit the further testing, production and deployment of nuclear weapons. Under SALT II, some nuclear weapons would be allowed limited additional production, others would be allowed limited technological improvements, some would be frozen at present levels, still others would be banned entirely.

If all three plans were available for implementation today, the United States would gain the most by adopting the arms limitation treaty, the freeze, or—preferably—both. START is a very weak third choice on its merits, let alone its negotiability. Yet the Administration bitterly opposes both SALT II and the freeze, arguing that they would undercut the strategic arms reduction talks.

To understand why sacrificing SALT II and the freeze to save START would be like throwing gold overboard to save brass, we can take the Administration's objections to SALT II and use them as yardsticks for measuring all three plans:

- *SALT II does not limit the Soviet Union's Backfire bomber.* This is the only specific objection to SALT II ever raised by President Reagan himself. In fact, SALT II limits production of the Backfire to 30 per year. The freeze limits it to zero. START does not limit the Backfire bomber.
- *SALT II allows the Soviet Union to retain 308 heavy intercontinental ballistic missiles.* SALT II and the freeze each allows the Soviet Union 308 heavy ICBMs; START allows 425. SALT II and the freeze limit the throw-weight—the warheads and their vehicle—of these missiles to 16,000 pounds each; START does not limit throwweight.
- *SALT II lets the Soviet Union produce an unlimited number of missiles and quickly reload their silos with them.* While SALT II does not control production of missiles, it prohibits development, testing and deployment of rapid-reload equipment. The freeze prohibits all missile production, although it has no effect on existing missiles or their reloading equipment. START does nothing about their missile production or reloading equipment.
- *SALT II counts launchers, which do not matter. START counts missiles and warheads, which do.* In fact, START does not count total missiles or warheads, which cannot be verified. It counts "deployable missiles," which are the same as launchers. SALT II also counts total launchers and warheads per launcher, which by simple multiplication is the same as START's "deployable warheads."
- *SALT II does not solve the problem of the vulnerability of U.S. ICBMs.* True, but neither does START, and the freeze does. SALT II and START each allows the Soviet Union to have highly accurate, well-tested multiple-warhead missiles that can each destroy several U.S. ICBM silos. But by prohibiting testing of these missiles and lowering confidence in their reliability, the freeze solves our ICBM vulnerability problem, while START does not.
- *SALT II permits the arms race to continue.* True, but it limits the arms race more than START does. Only the freeze ends the arms race. For example, the freeze allows no new types of ICBMs while SALT II allows one for each side. But START allows an infinite number of new types of missiles.

None of this is to say that START is worthless. It would be a modest, constructive addition in the context of SALT II, the freeze, or both. But in no sense is it a replacement for either.

Why, then, is the Administration pursuing START and opposing SALT II? There can be only one answer: the supremacy of political considerations over national security considerations.

The Reagan Administration came to power on a platform describing SALT II as "fatally flawed," although it

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was and is unable to say what those fatal flaws are in comparison to the only near-term alternative, which is the absence of strategic arms control. Once in power, the Administration actively sought ways to violate SALT II but was unable to find any, since the treaty had been carefully drafted to accommodate all ongoing U.S. strategic programs. At the same time, it was clear that the responsibilities of government imposed certain restraints not present on the campaign trail; specifically, clear renunciation of SALT II would free the Soviet Union from a number of significant restrictions. So the Administration has chosen the logically absurd course of observing SALT II while refusing to ratify it—thus allowing the Soviet Union to keep approximately 300 strategic weapons ratification would force them to retire. It is interesting to note that the destructive power of these 300 weapons is by many measures comparable to that of the 300 SS-20 intermediate range ballistic missiles which Eugene Rostow believes are causing "panic and terror" in Europe. The principal difference is that the SALT-controlled weapons can reach the United States while the SS-20s cannot. In national security terms, it is impossible to explain why the Administration should be deeply concerned about the SS-20 force, while refusing to exercise the stroke of the pen which could retire a comparable strategic force.

Opposition to the mutual and verifiable nuclear weapons freeze is more complex. The Administration made no campaign commitments for or against the freeze, which did not become an issue until after the election. But the Administration did have a commitment to "redress the strategic balance"—that is, to improve our nuclear force strength relative to that of the Soviet Union.

As readers of this journal are aware, the most highly publicized U.S. weapons planned for the next decade—MX and Trident II—advance over their predecessors principally in terms of first strike capability. When feasible Soviet weapons programs are considered it is not clear that 1992 will find our retaliatory capability to be of higher confidence than it is today, and it may well be less. Soviet retaliatory capability will almost certainly be less. Thus, after the balance is "redressed," the incentive to strike first and the danger of nuclear war will be higher than it is today. The only question is how much higher.

By preventing testing and deployment of new first strike weapons, and by preventing the confidence-testing of existing first strike weapons and thus lowering their reliability, the nuclear freeze would cause the probability of nuclear war to go down rather than up. But arguing this point with Administration policy-makers is an exercise in futility. They don't think in those terms.

Both before and after his election, Ronald Reagan has opposed every negotiated arms control plan to come to his attention. His major arms control appointments—Eugene Rostow, Edward Rowny, Paul Nitze, Richard Perle and Richard Burt—were chosen solely because of their opposition to the most comprehensive strategic arms control yet negotiated: SALT II. It is not realistic to expect other than unremitting opposition to arms control from the United States government so long as the Reagan Administration remains in power. □

HAROLD P. GREEN

The peculiar politics of nuclear power

In this year of nuclear anniversaries a look backward at how things were can help us to assess what they are today and what they might become. But our hindsight, to be instructive, should include the glaring errors along with the great events.

In retrospect, the physical control of a nuclear chain reaction seems to be a simple and obvious task. Yet the social control of that phenomenon—nuclear regulation—seems more complex and elusive than ever, and the passage of time only seems to compound the confusion.

It is fashionable today to call for the deregulation of practically everything, as if it were possible to wipe away all that has gone before and return to some simpler condition. In fact, what is needed for nuclear energy is not deregulation but reregulation, a rethinking and reappraisal of the atom's unique challenges for today and tomorrow.

As Harold P. Green points out, the struggle to regulate the atom has produced compromises that could balance competing social pressure only temporarily. Creating special authorities, such as the Atomic Energy Commission and the Joint Committee on Atomic Energy, invited social and political reactions that would ultimately lead to their destruction. Unfortunately, these special authorities had to become visibly troublesome to many members of the public before they could be abolished.

The same is true with the special authority created to protect atomic secrets: the doctrine that all knowledge concerning the atom is "born classified." For, as Mary M. Cheh notes, a censorship policy that excluded the public so completely could only survive for a while. The need to know and right to know were at odds, but this conflict could only be resolved when the public's stake in nuclear regulation—both military and civilian—became an issue in itself.

George L. Weil, the physicist who pulled out the final control rod to create a self-sustaining chain reaction 40 years ago, sees a fundamental change that has occurred in the regulation of the atom. "Back then," he said, of the 1940s, "there was no 'us-and-them' mentality between the scientists and public. There was only 'us'; we were the only ones who knew."

By contrast, Weil says, "an 'us' and 'them' conflict is just as meaningless today because the 'us' is the public"; the atom has become common knowledge to scientists and citizens alike.

Special authorities, like the Atomic Energy Commission and the Joint Committee, and special doctrines like the "born classified" policy, are remnants of a transitional period that has passed. If nuclear regulation is to succeed as sound public policy, it must recognize that today "us" means all of us.—William Lanouette, a free lance writer in Washington, D.C.

Somewhere, somehow, the dream of the 1950s and early 1960s that nuclear power would become the predominant source of electricity by the end of the century was shattered.

Forty years after the first chain reaction, it is useful to ask what happened to this dream and why. And it is tempting to answer these questions by asserting that almost every government policy decision relating to nuclear power since the Atomic Energy Act of 1954 has been wrong; that everything about the institutional structure of nuclear power is perversely inverse.

Although such assertions would not be too far amiss, the fact is that the causes of the present plight of nuclear power are complex and warrant more detailed consideration. The dominant fact is that nuclear power evolved in, to put it mildly, a peculiar political environment that shaped the development of the new technology along abnormal, if not aberrational, lines.

The entire atomic energy program existed within a closed political system dominated from 1947 until the mid-1970s by the powerful Joint Congressional Committee on Atomic Energy, which operated symbiotically with the Atomic Energy Commission for the greater power of both and for nuclear power for the public.

The Atomic Energy Act of 1946 created the Commission and vested in it extraordinary powers to operate in esoteric areas of science, subject to intense security-imposed secrecy. To counter-balance the Commission's power, the Act created the Joint Committee and vested it also with extraordinary power.

The Joint Committee skillfully used its asserted unique knowledge of nuclear science and its unique access to classified information to make itself the surrogate of the entire Congress rather than functioning, like other congressional committees, as the

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agent of the House or the Senate. Also unlike other congressional committees, which characteristically act to curb and restrict the agencies over which they have legislative jurisdiction, the Joint Committee was almost always more aggressive and expansionist than the Atomic Energy Commission and the Executive Branch, and it constantly pressed for larger and more ambitious atomic energy projects.

Moreover, the Joint Committee exercised its power largely through extra-legislative means, using the threat of legislation to ensure responsiveness by the Executive Branch to the Committee's desires. When legislation was required, Congress usually rubber-stamped the recommendations of the Committee, which was until later years generally regarded within the Congress as an elite body blessed with access to and comprehension of scientific information and secrets beyond the ken of ordinary congressmen.

The principal effect of the Joint Committee's role was that many basic decisions concerning atomic energy were made through negotiation behind closed doors and then publicly announced with an agreed-upon rationale that concealed important policy issues. Even where legislation was required, the details were usually settled in advance so that bills could be presented to Congress as non-controversial and with minimum exposure of underlying policy issues and differences of opinion.

The 1954 Act is itself a good example of this process. The legislation — by opening the door to a private nuclear industry—reversed by 180 degrees the atomic energy "island of socialism" established by Congress only eight years earlier. Thus, while the Act involved basic issues of public policy, these were scarcely discussed. Despite the fact that the 1954 Act reflected, in no fewer than two dozen references to "health and safety of the public," an obsession with the risks of nuclear power, nowhere in the published legislative history is there any discussion of the nature and magnitude of these risks.

This silence on basic public policy issues existed despite a voluminous legislative history and the longest Senate filibuster to that date. Unfortunately, the history focused on relatively trivial matters: a contract that President Eisenhower ordered the Atomic Energy Commission to negotiate with private interests for a power supply (that otherwise would have come from the Tennessee Valley Authority) to a new Commission facility in Paducah, Kentucky (the Dixon-Yates controversy); whether atomic energy patents should be subject to compulsory licensing; and whether consumer-owned utilities groups should have preference and priority with respect to nuclear power projects and electricity produced through use of nuclear power. More fundamental issues were considered and resolved in executive sessions of the Joint Committee, the transcripts of which, almost 30 years later, have not been released; and in less formal, off-the-record negotiations between the Committee and the Commission. The object, successfully attained, was to keep the basic issues from being openly discussed and to be able to present the principal features of a bill to Congress and the public as non-controversial.

One consequence of this abnormal political environment is that, although it is clear that the country had made a national policy commitment to nuclear power as the principal source of electric power in the future, there is no legislation in which this is stated. Thus, unlike most major domestic policy decisions, which are reflected in legislation resulting from intense public discussion and congressional debate, the commitment to atomic energy, like Topsy, "just growed."

Another consequence is that government policies, in both the Executive Branch and in the Congress (that is, the Joint Committee) were designed to protect the atomic energy enterprise from unfriendly criticism and scrutiny. By the 1970s, however, the atomic energy enterprise was struggling against hostile criticism and savage attack as the environmental movement grew and the power of the Committee waned. Like an overprotected child who cannot cope with real-world problems of adolescence and adulthood, so nuclear power was unable to make its way when it was exposed to the rough-and-tumble of ordinary political forces.

Institutional and economic forces. The 1954 Act represented an effort to "normalize" the atomic energy industry. Implicit in this normalization was the premise that investor-owned utilities would, subject to conventional state utility regulation, be the primary owners and operators of nuclear power plants. This meant that the substantial economic risk inherent in investment in a new, untested technology would fall upon utilities which traditionally were not accustomed to taking such risks. Moreover, the utilities were required to take on new personnel capable of managing this technology and of seeing it through the federal licensing process.

On the regulatory side, state utility commissions, hitherto concerned almost exclusively with issues of service and rates to consumers, had control over utilities' decisions to invest in nuclear power facilities; but they had no control whatever over the evolution of federal health and safety regulatory requirements, which had a major impact on the economics of nuclear power plants.

In the early post-1954 years, before the "energy crisis" emerged, the nuclear power program in the United

Congress regarded the Joint Committee as an elite body blessed with access to and comprehension of scientific information and secrets beyond the ken of ordinary congressmen.

States was not driven by a domestic need for the technology. Rather, national policy favored domestic nuclear power projects as a matter of international prestige and for positioning the United States as a supplier of nuclear supply systems to other countries. In any case, domestic utilities and reactor suppliers responded affirmatively to the summons, even though they recognized that nuclear power would be economic only at some future time.

Their commitment was, however, made somewhat easier by a variety of federal props and subsidies that served to encourage investment in the technology. These included federal contributions to research and development, a government commitment to ensure a supply of nuclear fuel and various necessary services, and a benign regulatory regime. As a result of these supports, and those provided by the Price-Anderson Act, discussed below, many of the real costs of nuclear power were not internalized. By the mid-1960s, the costs of nuclear power were, or were said to be, competitive with alternative modes of producing power.

With the rise of the environmental movement in the late 1960s nuclear power came under heavy political attack. Continuation of these subsidies became untenable and many were eliminated or reduced. At the same time, there was a sharp increase in the number and intensity of interventions in individual licensing cases, exacerbated by numerous complex new issues under the National Environmental Policy Act. As a result, the licensing process became dramatically longer and more uncertain. Finally, these factors, coupled with new information that compelled the Atomic Energy Commission and its successor, the Nuclear Regulatory Commission, to become somewhat less optimistic about health and safety questions, led to new and more onerous regulations. The cumulative effect, along with inflationary pressures, drove the capital costs of nuclear power plants sky-high. Plants originally estimated to cost some $200 to $300 million now were being completed at costs as much as ten times more, and for plants under construction the estimated costs per kilowatt were four or five times those of plants already in operation. These harsh economic realities are a major cause of the present moribund condition of nuclear power in the United States.

Nuclear safety. Central to the problem that has led to the present plight of nuclear power is the question of acceptability from the standpoint of health, safety and environmental considerations. This is not so much because of the reality, uniqueness or magnitude of risk, but rather because nuclear safety is a dominant element in both the economics of nuclear power and in the licensing process.

The technology involves two basic kinds of risks:

• It produces low-level radioactive effluents that must be discharged into the environment, and high-level radioactive wastes that must be stored in isolation from the environment for very long periods of time.

• A reactor accident might cause the release of large quantities of deadly fission products into the environment.

The real question is whether these risks are acceptable as a matter of public policy, and the answer to this question is intrinsically a legislative judgment on whether the benefits of the technology outweigh the costs and risks.

Unfortunately, because of the unique political environment discussed above, the United States has never had a "great debate" on the acceptability of the risks. For this reason, public discussion of nuclear safety has proceeded from polar extremes.

The position of the nuclear power establishment, industrial and govern-

Plants originally estimated to cost some $200 to $300 million now were being completed at costs as much as ten times more. . . .

mental, is that nuclear power, in principle, is acceptably safe because the 1954 Act, reaffirmed in subsequent legislation, holds that nuclear power plants should be constructed and operated. In this view, the role of the federal regulatory agency is to ensure that specific risks in particular plants are not excessive.

The opponents of nuclear power take the position that nuclear power is too dangerous to exist on the face of the Earth. Moreover, since the political process has been effectively closed to them they have sought to press the unacceptability of the risks during individual licensing proceedings.

The nuclear power establishment has displayed an Achilles' heel on the safety issue. During legislative consideration of the 1954 Act, the question of protection of industry against enormous potential public liability— for which insurance was not available —in the event of a serious accident was suppressed in order to avoid alarming the public. In 1957, however, in the euphoria of the nascent nuclear power future, the Price-Anderson Act was adopted by Congress. This legislation took into consideration the possibility of a nuclear power accident that could result in destruction vastly greater than ever before experienced in an industrial context; it provided for a combination of mandatory insurance, a half-billion-dollar government indemnity, and a total cut-off of liability at $560 million. This legislation, renewed for successive ten-year periods in 1967 and 1977, has provided opponents of nuclear power with the ability to argue, with over-simplication, ignoring the fact that Price-Anderson also provides important financial protection to the public and that nuclear power technology involves extraordinary risks whose economic consequences its practitioners were unwilling to assume, and whose physical dangers would have had to be assumed by people living in the vicinity of a power reactor.

Regulatory credibility. Especially after the weapons-test fallout controversies of the mid-1950s, the Atomic Energy Commission had a serious credibility problem, usually characterized as involving a conflict of interest among its various operational, promotional and regulatory functions. The solution had for many years been perceived to lie in splitting the Commission into two components, one of which would be a licensing and regulatory agency with no responsibility to promote the nuclear industry. Such a split was effected in the Energy Reorganization Act of 1974 which abolished the old Commission and created two new agencies: the Energy Research and Development Administration, which later became the Department of Energy; and the Nuclear Regulatory Commission. Although the 1974 Act explicitly required the latter to eschew promotion, most knowledgeable observers agree that those aspects of the defunct Commission's performance which had implied a promotional bias in licensing and regulation were continued and perpetuated by the new Commission.

This suggests that there is another explanation for the credibility problem. Since both agencies were staffed primarily with individuals from the scientific/engineering disciplines, they were highly intolerant of attacks on nuclear power where the attacks were based on rumor, half-truths and palpable distortion of scientific fact. The political process recognizes that even such mistaken views must be considered—because the holders of such views vote. But for purposes of regulatory judgments on acceptability of the risk, the Atomic Energy Commission and subsequently the Nuclear Regulatory Commission, regarded it as their sacred duty to prevent the public's being deprived of the benefits of nuclear power because of opposition based on erroneous, and often irrational, premises.

It is important to understand, however, the nature of the "scientific fact" being upheld by both Commissions. There had been no work with stationary land-based nuclear power plants before 1954, but with the aid of government subsidies, the technology advanced rapidly, without benefit of experience. The entire enterprise therefore rested upon scientific and engineering judgments that components, systems and human beings would, even in the absence of experience on which reliable prediction could be based, perform "as advertised." And if they did not, back-up systems were required, which were also expected to perform "as advertised." Accordingly, the risk of injury to the health and safety of the public was regarded as "vanishingly low."

In 1971, shortly after he became chairman of the Atomic Energy Commission, James R. Schlesinger likened the remarkable growth of the nuclear power industry from 1954 to 1971 to the hypothetical growth of commercial aviation from Kitty Hawk to the Boeing 747 in a similar 17-year period. The fact is, however, that the evolution of commercial aviation occurred over a much longer period of time with each successive step based on prior experience. Had this evolution in fact been compressed into a 17-year period, commercial aviation would probably be less safe or, at the least, the public would have less confidence in its safety.

The regulatory structure of nuclear power technology operated on two premises: that systems and people will behave as they are supposed to; and that the cognizant scientists and engineers can foresee and forestall adverse events. In other words, the licensing program was based on the omnis-

. . . harsh economic realities are a major cause of the present moribund condition of nuclear power in the United States.

cience and infallibility of the human beings who designed, constructed, operated and licensed the plants. Their judgment that something would work or turn out in a particular way was translated into a flat assertion of fact. But, since human beings are not, and cannot be, omniscient or infallible, the Commissions, in their turn, promised a greater degree of safety, and a lesser degree of risk, than could actually be delivered. Because of extravagant promises, the mere fact that the risks were in excess of zero was translated by opponents of nuclear power into the proposition that the dangers were excessive and unacceptable, even though they were modest, compared with other kinds of risk. Moreover, each successive revelation that nuclear power plants were really not quite as safe as previously promised further destroyed the credibility of the regulators and public confidence in their judgments.

The culmination of this process was, of course, the Three Mile Island episode, which showed the Nuclear Regulatory Commissioners and staff stumblingly attempting to cope with events that had never previously been regarded as plausible.

The licensing process. The 1954 Act established a detailed licensing scheme for a technology which then was not in existence. The heart of the scheme was a two-step procedure under which the applicant first received a construction permit and then, as construction neared completion, applied for conversion of the permit into an operating license.

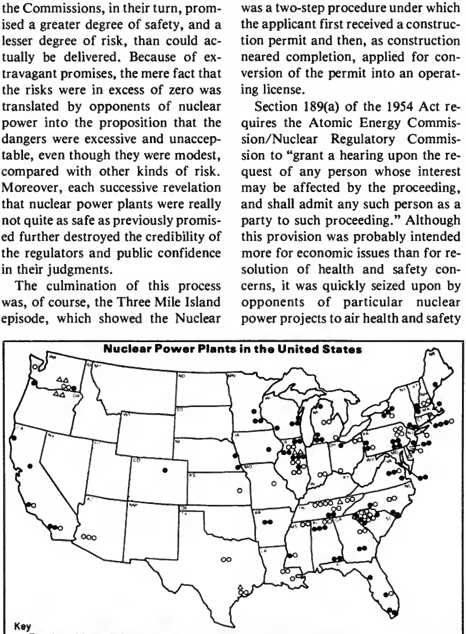
Section 189(a) of the 1954 Act requires the Atomic Energy Commission/Nuclear Regulatory Commission to "grant a hearing upon the request of any person whose interest may be affected by the proceeding, and shall admit any such person as a party to such proceeding." Although this provision was probably intended more for economic issues than for resolution of health and safety concerns, it was quickly seized upon by opponents of particular nuclear power projects to air health and safety concerns. To make matters worse, the Joint Committee's pique over the Commission's improvident 1956 grant of a construction permit to Power Reactor Development Company for the Fermi I reactor led to a 1957 amendment to the Act, making a public hearing mandatory for each license application, whether or not any matters were in controversy.

During the late 1950s and the 1960s, when there were few seriously contested licensing proceedings, the Commission used the mandatory hearing as a propaganda device to demonstrate the applicant's competence, the safety of the plant, and the thoroughness and competence of the Commission staff's review. More significantly, the staff's role in these hearings was to articulate and defend the decision it had reached before the hearing even began.

Beginning in the latter 1960s, when opposition to the licensing of nuclear plants became a way of life, the same hearing format was used. It is not surprising that those who opposed licensing the plants saw the Commission staff, rather than the applicant, as the leading proponent of the license, and this detracted even more from the agency's credibility.

These events coincided roughly with the enactment of the National Environmental Policy Act, which opened the hearings to a host of new, complex issues. Skilled trial lawyers entered the fray on behalf of environmental intervenors. The result was protracted hearings that strained the resources of the licensing agency and the applicants. The Atomic Energy Commission's response in the early 1970s was twofold: It extolled the value of "public participation" and encouraged interventions; at the same time, it changed its rules to make effective intervention more difficult and expensive. Environmental groups, lured by the Commission's rhetoric to

Nuclear Power Plants in the United States



Key
- ● Reactor with operating licenses
- ○ Reactor with construction permit
- ▲ Reactor on order

81	Reactors with operating licenses	62,705*
69	Reactors with construction permits	76,382
8	Reactors on order	8,886
158	Totals	147,973

July 1, 1982 *All figures in megawatts (electric)

Fundamental issues were considered and resolved in executive sessions of the Joint Committee, the transcripts of which, almost 30 years later, have not been released. . . .

intervene in the licensing process, became increasingly frustrated and alienated as they saw how the procedures had been rigged against them. With the weakening, and finally the demise, of the Joint Committee, they were able to acquire significant influence in Congress and in state legislatures.

The existing licensing process—long, arduous, uncertain and expensive—is certainly a major cause of the present plight of the nuclear industry. The two-step process may have made some sense in the early years of the technology, when one-of-a-kind power reactors were being built. Clearly, it makes no sense today, when most nuclear power plants are based on standardized designs. Moreover, the fact that a public hearing is mandatory at the construction permit stage, and will be held if a proper intervention occurs at the operating license stage, places the applicant at double jeopardy.

Public participation through adversary, judicial-style hearings, has become an integral part of the nuclear licensing process. Indeed, many members of the nuclear establishment find it difficult to speak of a nuclear power plant as "safe" unless it has been declared so after a public hearing. The fact is, however, that intervenors lack the financial and technical resources to make any constructive contribution to safety determinations. Their presence is probably counter-productive from the safety standpoint, since contesting them in a public hearing diverts scarce applicant and staff resources which can be more effectively deployed on real safety matters. Nor does the ease of public participation contribute to public confidence in the licensing process or the administrative agency, because the inevitable steamroller effect alienates, antagonizes and frustrates the unfortunate individuals who become involved.

These deficiencies have been well understood for at least the past 20 years, but no serious effort has been made to undertake basic reforms of the licensing system. Such reform proposals as have been made amounted only to small bandaids applied to specific perceived problems. And while such bandaids have usually produced some short-term temporary relief, in the long run they have made matters worse by further encumbering and complicating an already excessively complex process.

The reluctance to attempt really basic reform stems from the belief that it is politically infeasible. It is assumed, for example, that public participation is a "motherhood" issue, and any effort to curtail hearing rights, or to do away with the two-step licensing process—and thus reduce the number of opportunities for hearing—would produce substantial political opposition. Therefore, so the argument goes, a half-loaf that will see us through the months ahead is better than a vain effort to obtain the full loaf.

There may also be an unarticulated basis for the reluctance to press for a more rational licensing process. In the nuclear establishment's view, one advantage of the present system consists in the multiple levels of independent safety review. Successive reviews are performed by the applicant, the Nuclear Regulatory Commission staff, the Advisory Committee on Reactor Safeguards, the Atomic Safety and Licensing Boards, the Atomic Safety and Licensing Appeal Board, the Nuclear Regulatory Commission, the courts, and even the intervenors. These reviews moreover are performed twice—at the construction permit and at the operating license stages.

Bennett, United States

The multiple levels of review may be somewhat advantageous from the safety standpoint, but they also involve some negatives. For one thing, they support the employment of hordes of scientists, engineers and lawyers. They divert scarce technical resources to preparation for and participation in the public nuclear "show trials." And, most important, they involve a substantial diffusion of responsibility for safety determinations. No single individual or entity is required to "sign-off" on safety with the understanding that he, she or it will be accountable. There is always someone else to whom at least some responsibility for a debacle can be assigned.

One must conclude that, despite the costs in economy and efficiency, government officials involved with nuclear power technology are reluctant to press for fundamental change in the regulatory structure, particularly elimination of public hearings, because they obtain some comfort from the status quo.

Waiting for the miracle. Although the decline and ultimate fall of nuclear power has long been foreseeable, its adherents have kept the dream alive through the belief:

- that the opposition stems from a very small, if noisy, minority of know-nothings, and that an adequately educated public will support the technology;
- that strong pro-nuclear presidential leadership (which has not emerged in 20 years) will carry the day;
- and that a new energy shortage, causing blackouts and brownouts, will create a situation in which the public will beg for nuclear power.

Meanwhile, as the nuclear estab-

Public participation through adversary, judicial-style hearings, has become an integral part of the nuclear licensing process.

lishment has awaited one or more of these miracles, its strategy has been merely to survive, slapping on a band-aid here and there while the overall situation deteriorates.

The fact is that some segments of the nuclear industry have found it difficult to adjust to the realities of a harsh environment. They continue to search for the kinds of support that brought the industry through its first two decades. And they take great pride in the fact that to date they have beaten back anti-nuclear referenda and legislative actions, and continue to perform well in the polls. At the same time, however, the establishment has not been willing to test the political waters in the context of a proposal for basic reform of relevant federal policy.

This ostrich-like stance has produced no affirmative results in the past two decades, and it is unlikely that it will produce affirmative results in the future. Instead, the issue of fundamental reform must be faced and dealt with. Nuclear power, to succeed in this country, must survive its great public and congressional debate, and emerge with a clear and unambiguous national policy endorsement. The vehicle for this debate should be a proposal for a total overhaul and simplification of the Atomic Energy Act which, in relation to the domestic industry, should, at the very least:

- eliminate the present two-step licensing process in favor of a single license to construct and operate nuclear power plants;
- eliminate opportunities for opponents of nuclear power to delay or obstruct licensing proceedings in hearings;
- create mechanisms that will enhance the regulatory agency's credibility and public confidence in the licensing process;
- establish realistic regulatory standards and criteria for determining the acceptability of risk that will minimize the instability and uncertainty presently inherent in the process; and
- to the extent that nuclear power is determined to be essential to the national interest, establish a program of incentives designed to offset many of the current economic disincentives.

In view of the present level of public concern about and opposition to nuclear power, such a program obviously runs the risk of rejection by Congress. If that is the outcome, so be it. But if nuclear power has the virtues attributed to it by its supporters, those with faith in the democratic process must have confidence that truth will prevail. □

Secrecy: why is it still with us?

Imagine a law that declared an entire body of scientific knowledge secret and provided that no one could communicate these secrets unless the government gave its permission. Imagine that any person who violated the law could face a range of punishments up to and including life imprisonment. Imagine, too, that the scope of the law was so broad that, to enable society to reap the enormous practical benefits of the knowledge, many thousands of people had to undergo government investigation and clearance.

Few would believe that such a law could exist in the United States, with our strong commitment to individual liberty, free speech and freedom of scientific inquiry, and our great fear of dictatorial government powers. Yet such a law has existed in the United States for almost 40 years. It is known as the Atomic Energy Act and, by any American legal standard, the information control provisions of the Act are extraordinary.

Government secrecy existed long before the passage of the Atomic Energy Act in 1946. As early as the nineteenth century, directives requiring the protection of military information appeared in the United States and, by World War I, the markings of "Confidential" and "Secret" were already stamped on many documents. But atomic energy secrecy is like no other government secrecy.

A major difference is that atomic energy secrecy is based on statute. The statute dictates what is secret, who guards the secrets, and the circumstances permitting access to the secrets.

Conventional secrecy, with one minor exception, is based on executive order. The current order, E.O. 12356, establishes categories of national security information that are subject to classification; authorizes three levels of classification—"Top Secret," "Secret," and "Confidential"; and prescribes procedures for safeguarding classified data. The executive order, unlike the Atomic Energy Act, leaves government officials free to exercise independent judgment and to apply common sense in making security classification decisions.

Atomic energy secrecy is also radically different from conventional secrecy in the scope of what is classified as secret, the numbers of persons made subject to its provisions, and the procedures used to designate information as secret.

In scope, early secrecy dealt almost exclusively with military secrets such as numbers of weapons and location of troops. Even now, when the range of conventional secrecy has broadened from military data to national security data, the scope extends only to clearly specified information and only to information the government itself generates or controls.

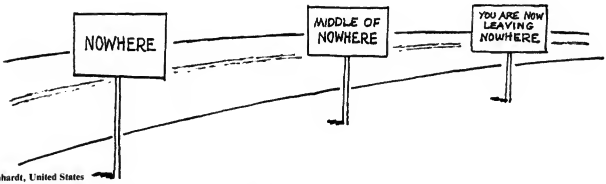
The rationale of limited scope is obvious. It is easier to keep secrets if you have few to keep or, as Justice Potter Stewart observed in the Pentagon Papers case, "when everything is classified, then nothing is classified."

The information control provisions of the Atomic Energy Act reject the narrow-scope approach. Rather, the law applies to all information falling within an open-ended and broadly stated concept of "Restricted Data" which is statutorily defined as:

"all data concerning (1) design, manufacture, or utilization of atomic weapons; (2) the production of special nuclear material; or (3) the use of special nuclear material in the production of energy. . . ."

In practice, this definition embraces virtually all atomic energy information the Department of Energy or the Nuclear Regulatory Commission believes warrants protection in the interest of national security. In the government's view, it does not matter whether the information is generated by the government or by a private citizen. "Restricted Data" can originate anywhere and, so long as it has not been affirmatively declassified by the government, it is secret. A class discussion in physics, the day-to-day reporting of events during the Three Mile Island incident in March 1979, speculation that the United States is developing new or different nuclear weapons, or studies showing workers in nuclear plants run higher risks of cancer—all contain information "concerning" the design, manufacture, or utilization of atomic weapons or the production or use of enriched uranium or plutonium in the production of energy. All come within the statutory definition of Restricted Data.

Atomic energy secrecy also differs from conventional government secrecy in the numbers of people potentially subject to clearance requirements. All other secrecy schemes limit not only the amount of information put under wraps but also positively



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limit access to individuals who need to have that information for government purposes. Again, the idea is that security is more easily and more reliably preserved if access is stringently confined, or as Ben Franklin observed, "Three may keep a secret, if two of them are dead."

At first atomic energy secrecy followed the rule of limited access. Classified information could be imparted only to those individuals who, of absolute necessity, had to have access in order to perform a function in a government program. And even then, such a person had only specific access to that information which he had a "need to know."

All this changed with the passage of the Atomic Energy Act of 1954, which envisioned and fostered the creation of a private atomic energy industry. It was obvious that private exploitation of atomic energy in the mid 1950s could not take place without easing the government's tight control over Restricted Data. But, rather than dictate the declassification of a wealth of information or narrow the definition of Restricted Data, the 1954 Act simply permitted the Atomic Energy Commission to broaden the class of persons to whom access would be given. After 1954 any person who could show a potential use of Restricted Data in his trade, business or profession became eligible—subject, of course, to having the requisite security clearance. The conventional idea of security linked to limited access gave way to the view that security could be maintained even if large numbers of persons had access to Restricted Data—even tens of thousands of persons—so long as all of them were investigated, cleared and subject to criminal sanctions for violation of security rules.

The final, perhaps most striking, difference between atomic energy secrecy and other government secrecy is the way in which decisions are made. In every other area information is withheld from the public only after there is an affirmative determination that that information must be kept secret in the interests of national security. Under the Atomic Energy Act, however, information constitutes Restricted Data by statutory definition alone. No affirmative classification by the Department of Energy or the Nuclear Regulatory Commission is contemplated or authorized. The information is simply "born-classified."

The historical circumstances giving rise to the Atomic Energy Act explain, and perhaps justify, information controls with these extraordinary features. The atomic energy information controls were, after all, first enacted at a time when the nation, including its legislators, were literally obsessed with secrecy. In 1946 atomic energy was new, complex and little understood. Only the atomic scientists and military personnel who had worked on the Manhattan Project had any depth of understanding or sophistication about its problems or potential. The public knew only that this unfamiliar force was harnessed to make a bomb of unimaginable destructiveness and that the United States was the only nation with the "secret" of that bomb. Almost everyone believed that national security and world stability turned on keeping other nations from learning our atomic energy secrets.

It was in this climate that Congress passed the Atomic Energy Act of 1946. The Act simply carried forward the wartime emphasis on secrecy and the government monopoly over all aspects of atomic energy research, development and application. Although Congress refused to vest the military with the oversight or control position it lobbied for, it did create a separate, civilian-controlled body, the Atomic Energy Commission, and empowered it to develop atomic energy under strict secrecy guidelines. Congress also created the Joint Committee on Atomic Energy which, because of its perceived expertise in atomic energy science and its access to atomic energy secrets, made far-reaching, closed-door decisions which were tantamount to final Congressional judgments.

From this early period on, the history of atomic energy shows notoriously little public debate about the work of the Atomic Energy Commission or the Joint Committee. Not only was there little opportunity for public review because of the strict secrecy regime, but also the press and the public simply continued the wartime habit of not asking any questions. The pattern of self-censorship was apparently deeply ingrained. Even among reporters it was thought somehow unpatriotic or inappropriate to inquire too deeply into atomic energy matters. There was also a widespread belief, fostered by the experts and the insiders, that the issues raised by atomic energy were, in any event, too impe-

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netrably complex for the uninitiated.

Thus, from early on, the wall of secrecy built by the Atomic Energy Act was continually strengthened and extended by circumstances. Among these were the tendency of the Atomic Energy Commission and the Joint Committee to exclude others from their decision-making processes; the esoteric nature of the basic scientific data, which led many to believe that only experts could comprehend the issues; and the general tendency of the press and public to avoid involvement.

Much has changed since the late 1940s and early 1950s. For one thing, atomic energy information has ceased to be important only to the military. Many countries, including our own, use atomic energy for the production of electricity and for a wide variety of other commercial and medical purposes. For another, the United States no longer has a monopoly over atomic energy "secrets," as witnessed by developments in the Soviet Union and other nations.

Another change is that no government, including the United States', now maintains a monopoly over atomic energy information. After 1954, the United States not only admitted American private interests into the field, but, acting through programs such as "Atoms for Peace," it promoted and encouraged the widespread sharing of non-weapons atomic energy information at home and abroad. There began an extensive declassification of Restricted Data and a worldwide exchange of atomic energy information among scientists, industry and government officials. Thousands of private citizens were given access to Restricted Data. The aim of all of this was to permit domestic and international development of the peaceful, commercial uses of atomic energy. But, because it was difficult, if not impossible, to separate information needed for commercial use from that needed for military use, it was inevitable that militarily-useful data was compromised. The result is that a spectacular amount of atomic energy data is now freely available. For example, public information about fission bombs is now so comprehensive that a 1978 *Bulletin* article concluded that "all the basic information for the design and construction of a wide variety of fission explosives has now been published in the open technical literature."

A final difference between today and yesterday, most experts would agree, is that now, nuclear non-proliferation no longer depends on information control but on materials control: control over trained personnel, industrial capabilities and, most importantly, over nuclear fuel, namely, plutonium and weapons grade uranium.

In short, the circumstances that explained and perhaps justified passage of the extraordinary and unique information controls of the Atomic Energy Act no longer exist. Why, then, do they continue in force?

It is difficult to isolate particular reasons. Plainly, the law's vitality does not rest on widespread public support because people are either indifferent or, far more likely, uninformed. Public reaction to the *Progressive* case is instructive. In 1979 the Department of Energy invoked the information control provisions of the Atomic Energy Act and successfully enjoined the *Progressive* magazine from publishing an article about government secrecy and the proliferation of thermonuclear weapons. The article, "The H Bomb Secret: How We Got It, Why We're Telling It," roughly described the theory and design of a hydrogen bomb. Although all of the information was derived from publicly available data, a federal district court agreed with the government that it was nevertheless Restricted Data which could not be published without government permission.

Regardless of whether public opinion supported the government or *Progressive*, public discussion betrayed surprise at the very existence of a law that authorized prior restraint against the media. A kind of "Gee, I

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didn't know that" quality pervaded the entire debate. And there appeared to be a basic misunderstanding of the importance of the case. Continually, people linked the *Progressive* case to the Pentagon Papers case, as if each raised the very same issues. But of course the two are fundamentally different, and the differences go to the heart of the unique qualities of atomic energy information controls.

In the Pentagon Papers case the government was attempting to enjoin publication of its own information, namely, a multi-volume history of United States' involvement in the Vietnam War. The government had, albeit overzealously, officially classified the history as Top Secret, and it was trying to prevent broad dissemination of the contents of the documents following their theft and leak to certain newspapers. In the *Progressive* case, by contrast, the government was attempting to enjoin publication of a citizen's own information. The information was developed from open sources. It was not classified, not stolen, not leaked. It was privately generated and its only connection with official secrets was the government's view that it fell within the broad definition of Restricted Data and hence was "born classified."

Except for the atomic energy community itself, the experts, too, seem unaware of the existence or significance of atomic energy information controls. For example, in 1973 the Columbia University *Law Review* published a 158-page article which to date represents the most exhaustive treatment of U.S. espionage statutes and government controls over national defense secrets. In that piece, the authors devote a scant page-and-a-half to the Atomic Energy Act.

Aside from the lack of widespread public support or understanding, the information controls survive because there is a natural inertia that dooms the repeal of all but the most unwanted statutes. A legislator may be sufficiently opposed to a law that he will vote against passage, but rarely is he so opposed that he will fight to undo what has already been done.

And, apart from general public unawareness and inertia, there are special factors that insulate the atomic energy information controls from repeal or amendment. Two of these relate directly to the peculiar political and social environment in which atomic energy developed in the United States.

The first is that the public perception of nuclear energy, shaped by events and impressions of the 1940s and 1950s, has never outgrown the influence of that period. No matter what the current reality, in the public mind decisions about nuclear energy are still primarily military matters, which of necessity must be kept secret and which in any event are so technical and complex that they should be left to experts. Moreover, the basic concern is always, implicitly or explicitly, the fear of a nuclear holocaust. And for many, this possible consequence—somehow indiscriminately linked with all government atomic energy secrecy efforts—justifies atomic energy information controls as a special case requiring special rules.

As a corollary of this heritage, the public and its surrogate, the press, still seem influenced by the view that it is unpatriotic or unwise to ask questions about atomic energy. The behavior of the press has been most telling. In 1948 Herbert Marks, general counsel of the Atomic Energy Commission, noted that the press covered that agency and its news releases "rarely with more penetrating comment or follow-up than that which accompanies the society news." Some 30 years later, in 1979, when the *Progressive* announced that it would resist the government's attempt to censor the H-bomb article, many newspapers were sharply critical. The Washington Post, for example, called the action "John Mitchell's Dream Case" and the now defunct Washington Star dubbed it the "flawless case for censors." These newspapers reacted not to the merits of the case, the record of which was sealed, but to the nuclear weapons nature of the information. Their conclusion was automatic and reflexive. For many newspapers, discussion of how a hydrogen bomb works was just off limits, the First Amendment notwithstanding.

The second reason why the controls continue in force despite a changed reality is the banality of secrecy. The

Government officials have already shown interest in borrowing the 'born classified' concept and applying it to controls over international technology transfers.

entire atomic energy secrecy apparatus operates in such a broad, yet low key, ordinary and even enlightened way that it is difficult to think of it as evil or bad. There are no witchhunts, no prosecutions, no smashed printing presses. Until the *Progressive* case, the only other reported instance of the government using the Atomic Energy Act to censor a publication was in 1950, when *Scientific American* magazine reluctantly agreed to delete portions of an article about the hydrogen bomb. And, with respect to private enterprise other than the press, there are no reported cases of injunctive or prosecutorial actions to force compliance with the law. This could be so because the government has resisted litigating its authority in the face of a challenge, but available evidence suggests that it is so because private enterprise voluntarily accepts the information controls.

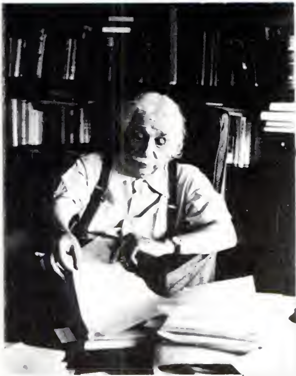
The system just seems to operate too smoothly and too efficiently to draw attention to the subtle harms it causes. One sees only a bureaucratic apparatus that commands and gets cooperation. And, although it stresses secrecy, it also appears to take seriously its statutory obligation, consistent with assuring the common defense and security, to permit and encourage the dissemination of scientific and technical information. One sees only an administrative system that has investigated and cleared thousands and thousands of people and, since its early period, from 1946 to 1953, discharges its security clearance function principally through informal commonsense assessments of who is and who is not a security risk, and in fact rarely denies clearance to those who seek it. One sees only scores of industry and research organizations accustomed to working closely with government security officials as members of the secrecy club and apparently doing well under a closed system.

In such circumstances, it is often hard to see how atomic energy secrecy actually hurts. But hurt it does. There are not only all of those hidden but real costs of mixing secrecy and science—chilling inquiry, compartmentalizing knowledge, driving out persons who seek a freer environment, and so on. There are also the many negative effects associated with a public purposefully kept uninformed or misinformed about an area of vital national concern. Atomic energy secrecy has led to a false sense of our own military superiority; has enabled the government to mislead us about the safety of atomic energy facilities and the effects of atomic weapons testing; and has even imperilled the future of atomic power in the United States. As a companion article points out, secrecy has, in part, prevented the great public debate so necessary for the public's ultimate acceptance of the risks associated with nuclear reactors.

There is also the harm created by the precedent of secrecy. Although the extraordinary secrecy provisions of the Atomic Energy Act are currently justified only in this one area, the precedent lies around like a loaded gun awaiting use in other spheres. Indeed, government officials have already shown interest in borrowing the "born classified" concept and applying it to controls over international technology transfers. It seems inevitable that the Act will serve as a model for another information control scheme.

On this anniversary of the first chain reaction, it is appropriate to ask for deeper explanations: Why do we continue to be ruled by the ghosts of atomic energy past? And why does the very breadth, efficiency and ordinariness of atomic energy secrecy continue to blind us to its evils? It is time to bring atomic energy secrecy in line with all other government secrecy or to justify anew its extraordinary features. □

Einstein's message to *Bulletin* readers -- 1952



BULLETIN OF THE ATOMIC SCIENTISTS

"*A Magazine for* SCIENCE AND PUBLIC AFFAIRS"

EUGENE RABINOWITCH
Editor

Dear Friend:

December 1, 1952

Ever since Hiroshima a group of scientists in our country has attempted to interpret for their fellow citizens and to the world the implications to our civilization of the emergence of atomic power. We have utilized our publication, the Bulletin of the Atomic Scientists, as the medium of communication. We have done this out of a spirit of dedication to the welfare of our community, which is the world, and in the belief that the truths of modern science, properly interpreted, can contribute to a better understanding of the new problems which confront the human race.

Over the years the scientists united in support of the Bulletin have had a few great successes and a reasonable number of failures. Through it all we have sought to view this new world objectively, as befits scientists, and to contribute to a solution of the great problems which exist between East and West, rather than to increase current tensions.

In response to a request by distinguished members of the Board of Sponsors of the Bulletin to make a public appeal to insure its continued publication, I ask your financial support in the conviction that few things are more important in the days in which we live than the search for those truths to which the Bulletin of the Atomic Scientists is dedicated.

I thank you for your interest in our common problem - the search for those truths which serve all mankind.

Faithfully yours,

A. Einstein

These words of Albert Einstein written thirty years ago to Bulletin readers continue to reflect the basic aims of the editors and the Board. We are conscious of this legacy and reaffirm our determination to preserve and strengthen the magazine as a useful instrument in the search for a peaceful, just and rational world.

To accomplish this goal we need your financial support. Each year our readers contribute to the Albert Einstein Fund which supports editorial projects that help us grow in influence and scope. Your contribution is tax deductible and will be of great help.

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How viable are ecosystems as they confront the revolutionary discoveries in nuclear physics and genetic engineering? These twin scientific feats, one at the core of matter, the other at the core of life, are without parallel in human experience.

LIEBE F. CAVALIERI

Twin perils: nuclear science and genetic engineering

There is a striking similarity between nuclear science and genetic engineering. Both major scientific accomplishments confer a power on humans for which they are psychologically and morally unprepared. The physicists have already learned this, to their dismay; the biologists, not yet. Molecular biologists are still euphoric over the potential of their achievements. Indeed, Nobel laureate David Baltimore has recently proclaimed: "We can outdo evolution"—a signal that molecular biologists are about to translate genetic engineering into an instrument of power much the way the physicists did when they exploited their discoveries at the beginning of the nuclear age.

The early part of this century was a golden era for physics. It was a time when physicists had become deeply submerged in finding the "truth" about the physical universe—a period of reasoned excitement that prevailed for a number of years; but it gave way to an urgent mood with the discoveries of the 1930s. There was no opportunity for the usual lapse of time between scientific discovery and its application: the discovery of nuclear fission defined the bomb. World War II and the threat of Nazi science provided an immediate rationale for physicists to tap one of nature's most powerful forces. "Truth" was converted instantaneously to power. Academic physics was to fuel the field of nuclear technology despite Ernest Rutherford's early disclaimer:

"A lot of nonsense has been talked about transmutation. Our interest in the manner is purely scientific, and the experiments which are being carried out will help us to a better understanding of the structure of matter."

But it was clear from the beginning that industrial and military uses of atomic energy were in the physicists' minds, for Rutherford also felt obliged to say:

"These transformations of the atom are of extraordinary interest to scientists, but we cannot control atomic energy to an extent which would be of any value commercially, and I believe we are not likely to be able to do so."

Leo Szilard and others did not share Rutherford's faulty judgment. The project went forward—the inexorable drive from truth to power was on. After reaching their goal, the physicists finally were left powerless with nothing to do but ponder, in a state of deep dejection, what they had done.

In the spring of 1945, Szilard wondered "what we were working for" when the Germans were close to defeat. "What is the purpose of continuing the development of the bomb, and how would the bomb be used if the war with Japan has not ended by the time we have the first bomb?" Much later, physicist Robert Wilson said:

"I would like to think now that at the time of the German defeat that I would have stopped, taken stock, thought it all over very carefully, and that I would have walked away from Los Alamos at that time. And I—in terms of all of my—everything that I believe in, before and during and after the war—I cannot understand why I did not take that, and make that act. On the other hand, it simply was not in the air. I do not know of a single instance of anyone who had made that suggestion or who did leave at the time. There might have been someone that I didn't know, but at the time it just was not something that was part of our lives. Our life was directed to do one thing. It was as though we had been programmed to do that, and we as automatons were doing it."

Frank Oppenheimer, brother of J. Robert Oppenheimer, has said:

"Amazing how the technology tools trap one, they're so powerful. I was impressed because most of the sort of fervor for developing the bomb came as a kind of anti-Fascist fervor against Germany. But when VE Day came along, nobody slowed up one little bit. No one said, 'Ah well, the main thing—it doesn't matter now.' We all kept working. And it wasn't because we understood the significance against Japan. It was because the machinery had caught us in its trap and we were anxious to get this thing to go."

Clearly, physicists kept on going for all the obvious human reasons. Freeman Dyson summed it up accurately and concisely:

"I have felt it myself. The glitter of nuclear weapons. It is irresistible if you come to them as a scientist. To feel it's there in your hands—to release this energy that fuels the stars, to let it do your bidding. To perform these miracles—to lift a million tons

It was clear from the beginning that industrial and military uses of atomic energy were in the physicists' minds.

of rock into the sky. It is something that gives people an illusion of illimitable power and it is, in some ways, responsible for all our troubles. I would say this—what you might call technical arrogance overcomes people when they see what they can do with their minds."

But the physicists were naive. Scientific prowess faded into insignificance in the face of political power. Szilard and others tried to stop the use of the bomb on Japanese cities, but by then it was too late. The power to create the bomb, originally in the hands of physicists, led insidiously to a new kind of power. The military stepped in, superseding any possibility of decision-making by the physicists. Those who had created the technology in the first place were cast aside, left only to reflect on the inhuman consequences of their work. The feeling of the physicists was put most poignantly by J. Robert Oppenheimer:

"We knew the world would not be the same. A few people laughed. A few people cried. Most people were silent. I remembered the line from the Hindu scripture, the Bhagavad Gita. Vishnu is trying to persuade the prince that he should do his duty and, to impress him, takes on his multi-armed form and says: 'Now I am become death, the destroyer of worlds.' I suppose we all thought that, one way or another."

As nuclear physics was approaching its climax, the golden age of molecular biology was just beginning. DNA was shown to be the genetic substance in 1944; in 1953 another milestone was reached, when James Watson and Francis Crick proposed the DNA double helical structure, with its molecular basis for genetic continuity. This was the dawn of molecular genetics; a new paradigm was born.

A sense of repressed excitement continued to permeate academic biology until, in 1973, all hell broke loose. The discovery that it is possible to recombine specific segments of DNA from different sources paved the way for recombinant DNA technology, a basis for genetic engineering. Natural genetic barriers had been overcome, and it was now possible to rearrange the genetic elements of distant species, producing genetic determinants not seen before in nature: hybrid molecules of DNA. New biological domains, not theretofore readily accessible, were opened to investigation and exploitation.

The overwhelming potential of recombinant DNA technology was apparent from the outset. The excitement has spread like wildfire throughout the scientific and commercial communities. Recombinant DNA technology has been so widely promoted by scientists and the news media that industrial giants from all over the world have been induced to invest heavily in it. Genetic manipulation of microorganisms by the new techniques has proceeded rapidly and is now widespread. More than 150 genetic engineering firms, mainly oriented toward the design of industrially useful microorganisms, have formed in the last few years. The technology has been translated into economic power, and with it molecular biologists have become entrepreneurs, leaving the Ivory Tower far behind. The profound ecological, social and ethical implications of genetic engineering have been obscured by its marketability.

As in the development of nuclear physics, once the elegance of the new discoveries had been felt and the intrinsic beauty of that corner of nature revealed, the enticement for further investigation and exploitation proved irresistible. Indeed, the science and its applications are so closely coupled that they are virtually one and the same. Like the discoveries in atomic physics, truth and use are hard to separate. Molecular biologists have succumbed to intellectual and emotional forces just as the physicists did, with medical and economic pretexts in place of the Nazi threat. No one heeds the trap that befell the physicists: scientists can choose and control their experiments, but once the science has escaped from the laboratory, they are no longer in control.

Already, an academic-industrial complex has been formed. More and more, the direction of university research is open to influence by commercial considerations. In the normal sequence of events, large corporations will gobble up the smaller, scientist-controlled operations as soon as they prove profitable. Nobel laureate Walter Gilbert says that the genetic engineering venture will be different from other commercial forays—scientists will be in control. Well, perhaps, for a while. It would take far more profound social changes than that to prevent the transfer of power to the corporate structure. The fascination of the endeavor, the "glitter" of genetic engineering and the "illusion of illimitable power" are again in the air; after chemistry and physics, it's now biology's turn. Few have asked with Erwin Chargaff, "Have we the right to counteract, irreversibly, the evolutionary wisdom of millions of years, in order to satisfy the ambition and the curiosity of a few scientists? . . . My generation, or perhaps the one preceding mine, has been the first to engage, under the leadership of the exact sciences, in a destructive colonial warfare against nature. The future will curse us for it."

There was an attempt not to go all the way with atomic fission. In his initial apprehension, Szilard was clairvoyant when he tried to preserve his idea of a nuclear chain reaction by patenting it so that it could remain secret. And indeed it seemed at first that a similar social action would oc-

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cur with recombinant DNA technology. In a spirit of humanitarian concern, when molecular biologists first perceived the impact of recombinant DNA technology, they decided to convene a conference in 1975 to seek a *modus operandi.* The serious and sincere discussion at the outset concerned only the immediate hazards of the research; future effects of the technology did not enter the discussion in any meaningful way. No public health experts, ecologists, ethicists or philosophers were invited, and the questions discussed were those most intimately connected to the research projects of the molecular biologists in attendance. The discussion was, therefore, not broad-based and it revolved about the possible escape of a pathogen from the laboratory into the environment. No long-range ethical and social consequences were considered.

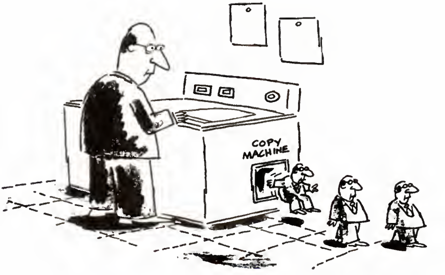
The ad hoc discussion at the 1975 conference served as a basis for guidelines for recombinant DNA research, first promulgated by the National Institutes of Health in 1976. Since then, the guidelines, under constant pressure from interested parties, have been relaxed to the point where they afford virtually no protection. In extricating themselves from government control, molecular biologists crossed an energy barrier and have become adept at political manipulation. The hope for meaningful public input on the ethical and moral implications of this technology has faded as the momentum of genetic engineering has grown.

Insofar as the scientific community has been distinguished by the purity of its motivation and its lack of bias and self-interest, to that same extent it has been free of corrupting power. But today power is thrust upon the scientist by the comprehensive knowledge he has gained, as well as by the vast technological influence of science in our society. To be true to itself, science must reject power in favor of responsibility. The scientist must have a conscience. Hand wringing after the fact offers no solution, as we have learned from the nuclear experience.

With the spectre of Nazism before them, physicists felt it necessary to develop atomic weapons. Molecular biologists, in choosing to push full-speed ahead with genetic engineering, cannot so convincingly rationalize their war on nature. In this period of unparalleled ecological stress, with so many socially important areas of inquiry unexplored, there is no overriding need to develop a technology that is pregnant with unpredictable consequences. All forms of life are vulnerable to this technology—any DNA can be connected to any other DNA; human DNA can be put into viruses and bacteria and vice versa; cancer virus DNA has already been put into bacteria, and so on. The gene pool of the Earth, the life-determinant of the future, is the experimental subject for genetic engineering. This precious, irreplaceable legacy of natural evolution is in the truest sense a one-time occurrence, and it would be naive to assume that we can manipulate it without harming ourselves. We do not have the requisite infinite wisdom.

In the face of the infinite complexity of natural systems, the idea that we could improve on the design of nature is not only hubris, it is frightening. In Lewis Thomas' words, we are ignorant "most of all about the enormous, imponderable system of life in which we are embedded as working parts. We do not really understand nature at all." We know that the Earth behaves like an indivisible, delicately tuned mechanism, in which the inanimate environment is strongly conditioned by living things, and vice versa; but we have only begun to decipher the influence of each part on the whole.

For example, we recognize that certain microorganisms convert organic wastes to usable nutrients, and that this recycling process is critical in maintaining the composition of the atmosphere and other conditions favorable to human life and to the web of species that sustain us. But we cannot predict the effects on these vital microorganisms of accelerated evolution, engineered by man, coupled with the accelerated environmental changes now produced by human activities—such as the production of carbon dioxide on a vast scale from fossil



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fuels, the distribution of novel chemical pollutants around the Earth, the large-scale clearing of forests, displacement of biological diversity by a minuscule number of cultivated species and so forth.

However, as the result of current efforts to design industrially useful organisms, microorganisms with properties taken from higher forms of life will inevitably escape into the ecosphere; other engineered forms will eventually be released intentionally into the environment for purposes such as the solubilization of trace metals in mining operations or the digestion of oil spills. We are laying the groundwork for unforeseen evolutionary changes that may create an environment inhospitable to present species.

Frequently, one is confronted with specious arguments about how well evolutionary forces have managed thus far and how they will continue to provide viable ecosystems. Certainly, we can find some assurance in nature's resiliency; life has survived environmental upheavals for millions of years. But as conditions have changed, so has the balance of life, with incompatible forms disappearing and new ones arising. If there were a drastic change in the environment, some forms of life would undoubtedly adapt, but humans, with their many, exacting biological requirements, could not evolve fast enough to become compatible with the new environment.

Genetic engineers have not overlooked the possibility of changing man himself. It will not be long before single-gene replacement therapy—the correction of a defective gene—will be possible. Although in this case the change will die with the patient, more radical experiments are underway in which eggs or sperm are altered to produce individuals with hereditary alterations. Considerable success along these lines has already been achieved in mice. The rationale for these experiments is that they provide information about mammalian genetics and fetal development. But when the technology for intervention in human evolution has been perfected, will it remain unused? Preliminary experiments with human embryos have been underway in England, for example, for several years. What is more seductive than the power to design human beings?

Although the repair of genetic defects appears laudable, the indistinct boundary between repair and improvement raises serious problems. Who is to decide what qualities define a perfect human? And even if there were a consensus today as to which genes are desirable, it certainly would not ensure that future generations would hold the same views. Clearly, the present cultural milieu is an important determinant of present values. In a changing world, the genetic engineering of perfection would imply a divine intelligence that could peer far into the future.

In *The Fate of the Earth*, Jonathan Schell poignantly describes the outcome of the use of nuclear weapons. Even if humanity were somehow to survive, the living conditions on Earth would be radically changed. The gene pool of the Earth would have been largely destroyed; diversity of living organisms, so important to ecological stability, would be minimized if not wiped out. Schell says:

"A full scale nuclear attack would devastate the natural environment on a scale unknown since early geological times, when in response to natural catastrophies whose nature has not been determined, sudden mass extinctions of species and whole ecosystems occurred all over the earth. . . . It appears at the outset that the United States would be a republic of insects and grass."

Because of human genetic damage, "Over the decades not only would the survivors of a limited attack face a contaminated and degraded environment but they themselves—their flesh, bones, and genetic endowment—would be contaminated: the generations that would be trying to rebuild a human life would be sick and possibly deformed generations."

Finally, Schell says:

"In weighing the fate of the earth and, with it, our own fate, we stand before a mystery, and in tampering with the earth we tamper with a mystery. We are in deep ignorance. Our ignorance should dispose us to wonder, our wonder should make us humble, our humility should inspire us to reverence and caution should lead us to act without delay to withdraw the threat we now pose to the earth and to ourselves."

In an initially more subtle way, genetic engineering also threatens to create an ecology that is inimical to life as we know it. In the effort to produce more "perfect" plants, animals or humans we may be headed for a monolithic, undiversified, unstable ecosystem—achieved, however, without a fireball.

The discoveries that energy can be released from the atomic nucleus and that DNA, the material of the cell nucleus, is the genetic stuff of life are without parallel in human experience. These twin scientific feats, one at the core of matter, the other at the core of life, demand a new consciousness if human life on this planet is to continue. We have mismanaged the applications of the first discovery. Now, as the second is about to be exploited, we must not permit the biosphere, surpassing as it does our understanding, to become an experimental subject. There is only one Earth, one earthly biosphere, and we are part of it. There is no margin for error. □

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To Die, To Live: The Survivors of Hiroshima, Robert Vas (writer and director), reviewed by John Dowling, Oct., 49.
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NOBEL PEACE PRIZE

The award of the 1982 Nobel Peace Prize to Alva R. Myrdal—longtime chief Swedish disarmament negotiator at Geneva and at the United Nations, and champion of women's rights—and to Alfonso Garcia Robles—architect of the Tlatelolco Treaty for a nuclear weapons free zone in Latin America and representative of Mexico and the Third World in all significant disarmament forums since the 1950s—is both appropriate and timely.

Between them the winners represent the vast, but usually silent and unrepresented majority of humankind, whose stake in avoiding a nuclear war between the major industrial powers is as great as that of the direct protagonists.

Never has the urgency been greater for the nuclear giants to recognize that, whatever their political and ideological differences, they have an overriding mutual interest in defusing their nuclear confrontation. But given the self-righteous arrogance of many of our leaders, it is necessary to propel them toward the negotiating table by overwhelming pressures, not only from within their own countries but from the rest of the world as well.

For their part in raising the level of understanding of and the pressure for action on nuclear disarmament and peaceful accommodation, Alva R. Myrdal and Alfonso Garcia Robles have more than earned their award. By giving the prize to them, the Nobel Peace Prize Committee has not only honored the recipients and their cause: it has gone a long way toward vindicating its own mission as well.—